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# American SOCIOLOGICAL Review

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## CHANGING IDEAS ON MENTAL ILLNESS AND ITS TREATMENT

JULIAN L. WOODWARD

*Elmo Roper, New York*

SOCIAL scientists sometimes get a mite discouraged over the slowness of the public to accept their findings. The folklore is tough and resistant, entrenched in "common sense" and bulwarked by dogmas concerning the nature of human nature. To substitute the viewpoints derived from scientific research seems a slow process.

However slow the process may be, it does eventually become apparent that the folklore is yielding in many areas. This paper presents some evidence that it is being overcome in the particular area having to do with mental illness and personal maladjustment. Folk beliefs concerning causes of mental disorder, folk attitudes toward the victims of such disorder, and folk prescriptions for treatment are all giving way to concepts and attitudes based on modern science. The change in a generation is really surprising, and should hearten the sociologists, the social workers, and the psychiatrists. This at least is the conclusion to be drawn from the research study about to be described.

In the summer of 1950 *Collier's* Magazine and the City of Louisville jointly agreed to finance a study of the attitudes of Louisville citizens on the general subject of mental health.<sup>1</sup> The study was one of a series

carried out by the firm of Elmo Roper at the behest of Mayor Farnsley of Louisville and generally designed to make citizens articulate with respect to the problems facing municipal administration.<sup>2</sup> In the mental health survey a cross-section sample of 3971 Louisville residents 18 years of age and over were personally interviewed in their homes by Roper interviewers. Interviewing took place in all the odd-numbered census tracts (44 in all) with quotas proportional in each case to the population of the tract. Within the tract the interviewer followed a prescribed route on a map designed to cover most of the tract area. She interviewed in pre-designated dwelling units, but could substitute the adjacent unit after one unfruitful call. Sex and age controls based on census estimates were used within each tract.

It is impossible within the confines of one article to report on all the questions asked respondents.<sup>3</sup> The writer will have to be content with stating some general findings and supporting each with one or more bits

<sup>2</sup> Julian L. Woodward and Alan Schneider, "What do the Citizens Think," *National Municipal Review*, 39 (June 1950), 288-292.

<sup>3</sup> A complete set of tables is provided in Elmo Roper, *People's Attitudes Concerning Mental Health*, New York, 1950. This document is unpublished but a limited number of copies are available for distribution to interested individuals and organizations.

<sup>1</sup> The committee in charge of the design of the project included Elmo Roper, Lawrence K. Frank, Dr. John Spiegel, Dr. William Keller, Charles Farnsley, Albert Maisel, and the writer.

of evidence from the study. Care will be taken to avoid stating conclusions where all the data do not point in the same direction. Inevitably there were some inconsistencies in the results from different questions. In the first place, people's attitudes are not necessarily consistent in areas where their knowledge is limited (nor in fact even where they are well informed). In the second place, there is always some error in measuring attitudes through the medium of questioning in a public opinion poll. But there was enough consistency to make certain conclusions fairly clear-cut and demonstratable.

## I

The first major conclusion that emerges from an analysis of the study results is that people (at least in Louisville) are definitely moving toward a humanitarian and scientific point of view toward mental illness, and have come quite a long way in that direction.

The old ideas that the mentally ill were bad and dangerous, and hence to be punished (on the one hand) or were ludicrous and silly, and hence to be laughed at (on the other) seem to be to a considerable extent superseded by the feeling that mental illness is a sickness that should evoke sympathetic understanding and that requires some form of professional treatment.

The general pattern of answers in the study as a whole is the best support for these conclusions, and the age breakdowns on nearly all of the questions show a clear-cut differential between the old and the young, with the latter uniformly the more "humanitarian." The young are also nearly always more "scientific" in viewpoint in the sense that they more often favor calling in professional help and are more attuned to modern sociological concepts of group therapy. The same contrasts appear in the breakdowns by education. Taken together

TABLE 1. Question: "There are all sorts of opinions about mental illness and what causes it. I'm going to read you some statements on which people differ and ask you to tell me whether *you* think they are more true than false, or more false than true."

	Total sample	Total males	Males Whose Ages Are:			
			18-24	25-44	45-64	65 and over
No. of respondents	3971	1839	260	842	543	188
Statement:	Per Cent Distribution of Replies					
"Most mental illness is inherited"						
False	72	71	75	77	65	52
True	16	19	18	16	21	29
Don't know	12	10	7	7	14	19
"Most hospitals for the mentally ill treat their patients very badly"						
False	46	49	57	53	44	34
True	22	22	20	21	27	20
Don't know	32	29	23	26	29	46
"There are not enough doctors and hospitals in Louisville to give proper care and treatment to all people who are mentally ill today."						
False	23	25	30	25	22	24
True	64	64	60	67	65	59
Don't know	13	11	10	8	13	17

TABLE 2. Question: "Which do you think is the best thing to do with sex criminals, send them to a hospital or a jail?"

	Per Cent Distribution of Replies								
	Total Sample	Education (both sexes)			Age (males only)				
		8th grade or less	High school	College	Total males	18-24	25-44	45-64	65 & over
Hospital	61	55	66	74	59	74	66	49	40
Jail	23	28	21	15	25	17	21	31	37
Other (volunteered answers)	8	9	8	7	9	4	8	12	10
Don't know	8	8	5	4	7	5	5	8	13

the analyses of responses by age and education of respondent provide convincing evidence of social change.

Tables 1, 2, and 3 provide good illustrations of the changes in attitude that have been occurring. A majority of people now doubt that "most mental illness is inherited," but there are fewer believers in the dogma of inherited taint among the young than among the old. The myth that "most hospitals for the mentally ill treat their patients very badly" is also rejected by a plurality of those with opinions, but the young age groups are somewhat more certain that the statement is on the whole an untrue one.<sup>4</sup>

The fact that the majority of Louisville citizens feel that there are not enough doctors and hospitals in the city to give proper care and treatment for the mentally ill is perhaps also an indication of growing sophistication. Younger and better educated<sup>5</sup> people

were more likely to hold this view and they were also much more likely to express the belief that the place for a sex criminal was a hospital rather than a jail. That 61% of the total Louisville sample favored hospital treatment for the sex criminal is the more surprising because a lurid sex crime had been on the Louisville newspaper front pages during the week preceding the interviews.

Finally there is evidence in Table 3 that the sense of stigma associated with mental illness is passing. About half of the Louisville citizens interviewed say they would not hesitate to tell friends and acquaintances about a family member who was mentally ill "just as if he had heart trouble or asthma." Faced with an actual situation not all these people would be as frank and revealing, nevertheless the expression of opinion is significant. It is especially so because of the age and education differences in response that are revealed in the Table. These differences are not as great as those on other questions—this is a question which strikes home and people are often more conservative about themselves than about others. But

<sup>4</sup> Age breakdowns are shown in the Tables only for males, to save space, but the female breakdowns were always similar in trend to those for males.

<sup>5</sup> 72% of college-educated people voted the statement true as against 67% of those with high school education and 61% of those who did not go beyond grade school.

TABLE 3. Question: "Suppose a member of your family became mentally ill. Do you think you would tell your friends and acquaintances about it just as if he had heart trouble or asthma, or would you try to keep it as quiet as possible?"

	Per Cent Distribution of Replies								
	Total Sample	Education (both sexes)			Age (males only)				
		8th grade or less	High school	College	Total males	18-24	25-44	45-64	65 & over
Tell it to friends	44	42	45	50	41	44	44	39	34
Keep quiet	47	50	46	45	50	47	47	53	54
Don't know and No Answer	9	8	9	5	9	9	9	8	12

the indicated direction of change is toward a more rational and scientific viewpoint.

## II

There were two case descriptions presented to respondents in the study in which a pattern of symptoms was described that an expert would recognize as strongly pointing to probable psychosis. How did respondents react when asked to suggest "what should be done" about these two subjects?

As a preliminary to discussing the cases individually one may make four generalizations that apply fairly well to both of them. It may be said (1) that there was a probable failure on the part of a large share of the respondents to recognize how seriously ill and in need of expert help the two subjects were. In spite of this there was (2) a substantial number of respondents who sug-

gested some form of "scientific" treatment, usually referral to psychiatrist or physician. Somewhat overtopping the "scientific" group was (3) a group favoring what might be called a "common-sense humanitarian" (but not scientific) approach. The size of this latter group is no doubt influenced by a failure to diagnose the need for professional treatment, by lack of knowledge of the existence of such treatment, and by doubts concerning its efficacy. Finally, as a fourth generalization, it should be noted that any punitive or "disgracing" treatment technique is in extremely low favor. This fact would also be more significant if the cases had been regarded as more serious.

Turn now to the first of the two cases for evidence to support these generalizations. The lady described in the question in Table 4 is a paranoid type clearly in need of professional treatment, and she may conceivably

TABLE 4. *Question:* "Mrs. B had always been a little suspicious and inclined to take the worst view of things, but she had led a fairly happy married life until she began to accuse her husband of not loving her any more. When she saw him speak politely to an attractive widow next door, Mrs. B waited until he had left, got hold of his gun, and then went over and threatened to kill the widow. Mrs. B's husband hadn't done anything wrong and doesn't know what to do about her."

"Here are some things various people have suggested might be done about Mrs. B. If only *one* thing on the list could be done, which one do you think it would be best to do?" (Card shown respondent)

Per Cent Distribution of Replies						
	Total Sample	Males Whose Ages Are:				
		Total	18-24	25-44	45-64	65 and over
Her minister or priest should be called in to talk with her	26	25	23	25	26	25
The husband should give her a good talking to and then wait to see if her jealousy won't blow over	21	23	34	23	18	19
The family doctor should be called to see if he can't give her something to calm her nerves	21	19	11	21	19	17
The husband should stay home with his wife to prove to her he really loves her	13	13	17	12	15	11
She should be taken to a mental hospital where she can be treated and where she can't harm anyone	7	7	5	7	7	6
The police should be called immediately to lock up Mrs. B until she calms down	1	1	1	1	2	2
None of them or Don't know	11	13	10	11	13	20



become dangerous to her neighbors if it is too long delayed. When read the case description and asked the open question, "What do you think ought to be done about Mrs. B?" less than 2% said, "Have her arrested," less than 1% suggested physical punishment, and 1% wanted her put away in a mental hospital. On the other hand 20% said, "Take her to a psychiatrist," and another 13% said, "Take her to a doctor."

Commonsense-humanitarian techniques were also popular. Fifteen per cent advised, "Talking it out and convincing her she is wrong," and 7% said, "Show her more love and understanding." When all the commonsense "home treatment" suggestions (Talk it out, get help from religion, "ignore her," show her more love, change her environment) are totalled, they bulk slightly larger than the "scientific-treatment" suggestions. Twenty-five per cent of the respondents would make no suggestions whatever on what should be done with Mrs. B.

When respondents were given a list of possible "things to do" about Mrs. B (see Table 4) and asked to say which one they thought would be best, the drastic, protective-punitive actions (lock her up, put her away in a mental hospital) found little favor. More than half of the people picked the alternative: "The police should be called immediately to lock up Mrs. B until she calms down" as the *worst* thing to do under the circumstances. Whether this unwillingness to put Mrs. B away was due to a failure to recognize her as really queer or dangerous or whether there was a complete loss of faith in the old protective approach to mental disorder is of course a question. The total pattern of evidence in the study would indicate that there was some of both types of thinking involved, as well as a strong disinclination to bring disgrace on anyone unless absolutely necessary.

As between the commonsense home-treatment approach to Mrs. B's case and the scientific-professional approach the results are clear-cut only in showing that both are preferred to the protective-punitive. Resort to the "minister or priest" was the most favored alternative and he is evidently re-

garded as a person to turn to in trouble. Nowadays he is more likely to have enough training to recognize a psychotic when he sees one, and to pass Mrs. B on to a psychiatrist. The same may be true of the family doctor. There is, however, a willingness on the part of a good many respondents to prescribe their own treatment for Mrs. B, presumably in terms of their own experience in human relations. Some 21% favor fairly drastic action by the husband in disciplining his wife by "giving her a good talking to" (the women are for this almost as often as the men). A smaller group (13%) think he ought to stay home more and prove that she is his major object of affection.

Much the same conclusions may be drawn from the case of Mr. G (see Table 5), a depressive, only here there is apparently less recognition of the need for professional treatment and more tendency to rely on kindness and commonsense. He should be given encouragement and understanding from family and friends; "someone" should get him a new job; he ought to "pull himself together" (lift himself by his bootstraps!) and find a new job for himself; he should have a rest or vacation; he should find some kind of distraction—a hobby perhaps or at any rate "something to do." These are the sorts of suggestions that respondents make frequently when asked the open question, "What do you think ought to be done about Mr. G?" Only 9% spontaneously suggest that he see a psychiatrist, an additional 8% recommend a doctor, and 6% propose "religious therapy" ("He needs more faith in God and prayer") through a minister or priest.

When the alternatives are organized for the respondent (Table 5) the results are not very different. Even when psychiatric treatment is specifically suggested it gets only 11% of the first choices (this rises to 24% among the college-educated). The field so far as Mr. G is concerned still largely belongs to lay diagnosis and commonsense. Of course a potential suicidal is not dangerous, and is a natural object of pity and a prime candidate for Christian charity and helpfulness. Perhaps it is a mark of progress that

TABLE 5. *Question*: "The last person I want to tell you about is Mr. G, a 52 year-old machinist. Mr. G had always been a hard worker who had worried a lot about making both ends meet for his large family. One day his job at the plant was given to someone else and he was told by his employer that he was no longer needed. After this happened, he became very depressed, accused himself of being a complete failure, and worthless to his family. He refused to look for another job or take an interest in anything and finally tried to commit suicide.

"Here are some things various people have suggested might be done about Mr. G. If only one thing on the list could be done, which one do you think it would be best to do?"

	Total Sample	Males Whose Ages Are:				
		Total	18-24	25-44	45-64	65 and over
No. of respondents	3971	1839	260	842	543	188
Per Cent Distribution of Replies						
His family and friends should give him a good pep talk and urge him to look for another job	33	34	41	35	29	32
He should go to his family doctor to find out if there is a physical illness that is causing him to feel badly	16	15	12	16	16	16
He should have a good long rest away from his family responsibilities and worries	15	15	17	15	15	11
He should be given plenty of time to recover from the shock of losing his job and then he'll be all right again	14	12	10	11	14	11
He should be sent to a psychiatrist for consultation and treatment	11	12	12	14	10	5
He should be sent to a mental hospital or asylum until he is better	2	3	1	2	3	6
None of them or Don't know	9	9	7	7	13	19

as many as 11% of the people of Louisville recognize that Mr. G is a mental case of sorts, and that someone called a psychiatrist might be more helpful than a pep talk or "long rest" in effecting a cure.

### III

The tendency to abandon repression and incarceration as treatments and to substitute more humanitarian and "scientific" techniques is especially exemplified in the case of the juvenile delinquent included in the study (Table 6). Here the sociologists and social workers seem to have really made a good start in selling the public on their point of view concerning the treatment of juvenile

crime. When asked the open question on what ought to be done about the fifteen-year-old truant and automobile stealer the most frequent responses are of the repressive type (punish him, send him to a reform school). But the minute the boy's club is suggested to them as one of six possible courses of action (Table 6) it commands clear majority support. The reformatory and the "old-fashioned whipping" retreat almost to the bottom of the list, behind juvenile probation and referral to a psychiatrist. And re-examination of the open-ended question answers indicates that while punishment and the reform school get the most spontaneous votes and head the list, the actual number of votes for more modern techniques,

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when all the different categories are added together, overtop those going to traditional repression.

The people who oppose repression have good reasons for their attitude, good at least from the sociologist's point of view. Those who think that whipping is the worst of the six alternatives say simply that a hiding would arouse vindictive feelings in the youngster and would lead to more of the same behavior—it would do no good and simply make him worse. Those who are against reform school emphasize especially the bad associations there, using such phraseology as the following to describe their feelings: "He would be with bad companions;" "He would learn more about crime from older inmates;" "There's too many boys get together and each boy has different troubles and they discuss them so they think next time I wouldn't get caught;" "If you throw a partly good apple in with rotten apples, both get rotten;" "Reformatories are crime breeders."

The opponents of reform schools also say that the youngster sent there would feel unloved and unwanted and get a grudge against the world. The reform school atmosphere would make him resentful and tough.

The extent of the change in the direction of modern ideas on juvenile delinquency is graphically portrayed in the age breakdowns in Table 6, and of course it is apparent in the education breakdowns also (not shown for lack of space). The older a person the more apt he is to support the "old-fashioned whipping" and the reformatory sentence and the less likely to advocate boy's clubs or psychiatric guidance. The gap between the oldest and the youngest groups is quite marked. The less education a person has had the more likely he is to follow the pattern of the older age group, except for one anomalous instance. The proportion among the college-educated group who chose the boy's club as the preferred treatment alternative is *less* than that among those with only a high school education. It may be that the

TABLE 6. Question: "I'd like to ask you a question about a fifteen-year-old boy who has been in trouble repeatedly for staying away from school and has recently stolen an automobile.

"Here are some things various people have suggested might be done about this boy. If only *one* thing on the list could be done, which one do you think it would be best to do?"

	Total Sample	Per Cent Distribution of Replies				
		Males Whose Ages Are:				
		Total	18-24	25-44	45-64	65 and over
See to it that he joins a boys' club and is encouraged in sports and other worthwhile activities	55	52	63	56	49	36
Have a psychiatrist find out why he behaves this way and then try to change his attitudes and behavior	25	22	23	25	21	12
Put him on juvenile probation and have a probation officer check up on him frequently	7	9	6	8	10	11
Send him away to a reformatory like Greendale	4	5	3	2	7	13
Have his father give him a good old-fashioned whipping	3	4	1	3	4	10
Punish his parents by sending them to jail if he does anything else that's bad	1	2	0	1	3	4
None of them or Don't know	6	6	4	5	6	14

college-educated recognize the boys' club as a preventive rather than treatment technique—when a boy actually becomes delinquent it may make more sense (they seem to be saying) to bring in a psychiatrist or a probation officer.

## IV

There have been some hints in the foregoing paragraphs that the psychiatrist is beginning to gain popular recognition as a resource in dealing with personality problem cases. Actually how much recognition does he have today, and in what context does he fit in the public mind? Is he regarded as a medicine man with a mysterious magic for curing personality disorders? Is he viewed as a special type of doctor, and trusted as a doctor is trusted? Is he regarded as a poor sort of beginner scientist but the best thing available at present? Or do people think he is an overcharger and a near-quack?

The study supplies several bits of evidence that bear on these questions. First, there are the responses to the true-false statement question on whether it is "always worthwhile to get a psychiatrist's help when someone begins to act queerly or get strange ideas?" The results (Table 7) indicate that the irreconcilable opposition to the psychiatrist is now relatively small—8 out 10 Louisvillians think that the psychiatrist's help is worth having when the person is pretty clearly a mental problem case, and education does not affect this ratio. Of course

the question is, in a sense, loaded a little in favor of the psychiatrist since a person *could* reason: "Psychiatrists probably can't help much but at least it won't do any harm to try them." But no one who is really anti-psychiatrist will fall in this category.

When the 19% who do *not* believe the statement (in Table 7) is true were asked *why* they felt that way, it was found that the active opponents of psychiatry were very few indeed. Table 8 shows the results after coding and tabulating these "why" answers. The largest group of respondents were simply loath to take such a drastic step as to call in a psychiatrist until other resources had been first exhausted. One of their reasons for this hesitance was apparently a feeling that the mere fact of reference to a psychiatrist would frighten the patient into thinking he was very ill. Doubts as to the competence and/or honesty of psychiatrists were expressed rarely and the people who would oppose calling in any psychiatrist because of expressed lack of faith in them as a group appear to be less than 1% of the total population.

A second kind of appraisal of the psychiatrist's position in public confidence is provided through the question responses shown in Table 9. Two issues, at least, are involved in choosing types of people to supervise a publicly financed program for mental health. First, there is the question of whether the program should be put in charge of "experts" or in the hands of respected but non-expert individuals. Second, there are the

TABLE 7. Question: "It's always worth while to get a psychiatrist's help when someone begins to act queerly or get strange ideas. On the whole, do you think this statement is more true than false, or more false than true?"

	Total Sample	Respondents Whose Formal Education Ended in:		
		Eighth Grade or Less	High School	College
No. of respondents	3971	1745	1589	533
Per Cent Distribution of Replies				
True	81	83	81	79
False	10	10	10	10
Don't know and No answer	9	7	9	11



TABLE 8. Question: "Why do you feel that it's not always worthwhile to get a psychiatrist's help when someone begins to act queerly?" (Asked of those who answered "False" or "Don't know" to the question in Table 7)

	Sub-totals	Total
No. of respondents	...	683
	Per Cent Distribution of Replies	
Should try other help first	..	34
Family doctor is better, should be consulted first	10	..
Minister or priest is better, should be consulted first	3	..
Family or friends are better, can help more	3	..
People should try to work it out themselves	3	..
Other (try other help)	2	..
Psychiatric help not necessary in many instances (some cases not serious enough: everyone is a little queer, etc.)	..	15
Make person worse to see a psychiatrist: create suspicion he is mentally ill	..	6
Don't believe in psychiatrists—don't have faith in them	..	5
Should only go to a psychiatrist if he was first-rate—many are not	..	4
Some people are afraid of psychiatrists	..	2
All other	..	6
Don't know and No answer	..	46

related questions of who is an expert in the public's mind and whom among the experts does the public trust?

Table 9 indicates that in general an "expert" committee is favored, and the more education the respondent has had the more he wants to see the mental health program run by professionals. It is true that the "priest, minister, or rabbi" heads the list of preferred committee members in the Louisville population as a whole, and is number two even among the college-educated, but it is clear from the study as a whole that this individual qualifies as an expert in the minds of many, and as a mental hygienist and counselor for the non-psychotic he is indeed often entitled to that status. Numbers two, three, and four on the list are the family doctor, the psychiatrist, and the social worker, and all of these fall in the expert category. Far down on the list come the businessman and the banker. There is a theory in our culture that the businessman is the one to be trusted if public money is to be spent effectively and efficiently on any project, but this theory seems to be rejected in this instance.

Within the roster of experts the psychiatrist's stock is clearly on the rise. Note how his support increases with decreasing age and increasing education. The social worker, the mental hygienist, and the psychologist all show the same pattern of increased prestige among the young and the educated, although with less total support than is given the psychiatrist. The family doctor pattern is the reverse, he loses support where these other experts gain it, but he still remains high on the list of choices for all sub-groups. As for the sociologist, the kindest thing is to say is that he is a "pure scientist" and doesn't belong on the committee anyway. But it is to be doubted that this was the reason why so many left him off their list!

There is a third bit of so-far-unreported evidence concerning the public's view of psychiatry and psychiatrists. People were asked whether the following statement was more true than false or more false than true: "The experts themselves often can't agree on whether a man is mentally ill enough to be put in an insane asylum or not." The statement is possibly loaded somewhat in favor

TABLE 9. *Question: "If a city or state government decided to spend a lot of money to help prevent mental illness, which four of the kinds of people on this list would you like to see on the committee that was to decide how to spend the money?" (Card shown respondent)*

	Per Cent Distribution of Replies							
	Respondents Whose Formal Education Ended in:				Respondents Whose Ages Are:			
	Total Sample	8th Grade or Less	High School	College	18-24	25-44	45-64	65 and over
A priest or minister or rabbi	62	60	67	58	63	62	63	57
A family doctor	60	66	59	53	55	59	64	65
A psychiatrist	42	29	52	63	53	48	36	21
A social worker	39	37	41	45	44	40	37	33
A mother	35	42	34	22	34	32	40	41
A juvenile court judge	33	36	32	34	31	33	34	34
A mental hygienist	24	18	30	30	34	27	19	15
A businessman	21	20	22	24	16	22	22	19
A school principal	16	21	12	11	15	15	17	18
A banker	11	12	10	10	8	11	12	12
A psychologist	8	5	10	15	11	10	6	4
A psychoanalyst	8	5	10	13	10	10	6	3
A sociologist	5	2	5	15	7	6	3	3
Don't know and No answer	7	9	3	2	4	5	8	16

of a "true" response (except for the word "often"), since many people have no doubt read of the disagreements between state and defense alienists in the big criminal trials. Nevertheless, 27% of the total sample and 31% of the college-educated declared the statement "false." Another 23% of Louisvillians would express no opinion as to its truth or falsity.

It is not easy to summarize in a sentence or two the conclusions on the position of the psychiatrist that one may safely draw from the results presented in this and the preceding sections. The psychiatrist is a relative late-comer in the ranks of guidance experts, and attitudes toward him are still undergoing fairly rapid changes. As a resource in dealing with clear-cut cases of "insanity" or extreme and obvious mental disorder, his status seems already to be well established. It is in connection with the borderline instances of "people in trouble" and "people who are a bit queer or neurotic" that his role is still undefined. It seems clear that Louisville citizens know much better when to call in a doctor to treat a serious physical illness than they do when to call a psychi-

atrist to deal with a serious mental illness. In the first place, the extent of the physical illness is easier to recognize; in the second place, the very fact of mental illness involves a stigma which makes people resist facing the fact of psychosis. Calling in the minister, the family doctor, or the best friend involves much less loss of face for the individual and his family than if a psychiatrist is consulted. It would seem to be one of the psychiatrist's major problems today to break down the "nut doctor" and the "last resort" associations in the public mind and qualify as a counselor, a mental hygienist, and a person who deals with the less serious personality problems, as well as with the candidates for mental hospitals, shock treatments, and the other drastic treatments that have been publicized in the magazines and the films.

## v

We have already seen that the college-educated group is well ahead of the rest of the population in its adoption of modern attitudes on mental health. This was to be expected. It was equally to be expected that the professional groups in the society would

also be "progressive" in viewpoint, partly because of formal education, partly because of contacts and experience. But which professional groups are most up-to-date in their viewpoints? Since the members of professions are likely to be influential in the community it is important to know which groups provide what sort of leadership on mental health.

In the Louisville study lawyers, doctors, school teachers, and members of the clergy were "oversampled" by selecting names systematically at random from lists of all such people in the city, and the extra interviews made with these people were added to the interviews which came about naturally in completing the population cross-section.

While the total number of interviews in each professional group was still relatively small (100-135) a good many of the comparisons made between the groups involved such large differences as to be statistically reliable. Table 10 summarizes some of these comparisons.

The only clear-cut, over-all generalization that emerges after an examination of the Table is that the lawyers are the most conservative and, from the mental hygienist's point of view, the least enlightened group. Lawyers are more likely to resort to repressive measures in dealing with juvenile delinquency and mental illness (there was not enough space to include all the evidence on this point), they show considerably less faith

TABLE 10. COMPARISONS BETWEEN RESPONSES OF DOCTORS, LAWYERS, TEACHERS, AND CLERGYMEN ON SOME QUESTIONS IN THE MENTAL HEALTH STUDY

	Total Sample	Lawyers	Doctors	Teachers	Clergymen
No. of respondents	3971	108	123	135	116
Per cent of respondents who favor:					
Sending the juvenile delinquent (Table 6) to a psychiatrist	25	27	49	54	33
Putting him on juvenile probation	7	18	5	1	6
Sending Mrs. B (Table 4) to a mental hospital	7	8	29	..	6
Calling in the minister or priest	26	13	17	..	50
Sending Mr. G (Table 5) to a psychiatrist	11	..	43	..	25
Per cent of respondents who believe that:					
Most mental illness is inherited (Table 1)	17	16	19	4	13
Most hospitals for the mentally ill treat patients very badly	22	31	18	19	22
The experts often can't agree on whether a man is mentally ill enough to be put in an insane asylum	50	76	50	53	63
It's always worthwhile to get a psychiatrist's help when someone begins to act queerly or get strange ideas	81	58	89	75	72
On the committee to spend public money to help prevent mental illness (Table 9) there should be a:					
Psychiatrist	42	60	80	70	54
Juvenile court judge	33	36	40	23	39
Businessman	21	68	51	22	19
School principal	16	8	15	10	7
If a member of the family became mentally ill they would <i>not</i> tell friends or acquaintances about it					
	47	68	47	50	46

in psychiatry than the other professional groups, and they are not much better informed on local facilities for care of mental patients than the population as a whole (again the evidence is omitted<sup>6</sup>). Since the lawyers as a group occupy a position of great power in our governmental structure, their opinions about psychiatry and the need for treatment facilities are of considerable importance. Neither their education nor their present contacts seem to be functioning adequately to keep them up-to-date on current trends.

The other groups (teachers, doctors, clergymen) do not differ so much among themselves as they all differ from the lawyers. The doctors not surprisingly show a stronger faith in psychiatry, and the clergymen in religion, than do the other groups. The teachers are most willing of all groups to see the juvenile delinquent have psychiatric treatment, and in general their position is somewhat closer to that of the sociologist or social worker than is that of the clergyman or doctor. The teachers do not show any special eagerness to have a school principal on the committee administering mental health funds—apparently they think that even a businessman would do a better job!

<sup>6</sup> It is planned to publish separately some results of questioning on knowledge about local facilities for the care of mental patients and attitudes toward the quality of treatment given.

## VI

This paper can report only a small part of the evidence from the Louisville study but it may be said that in general the findings on the unreported questions support the conclusions arrived at in the preceding pages. These conclusions may be recapitulated as follows:

1. The public has come a considerable distance in giving up old beliefs and superstitions about mental illness and in adopting more modern, scientific viewpoints.
2. There is still a gross failure to recognize serious mental symptoms, at least when they are described in words. The story may, of course, be different when the people themselves are under observation.
3. There is considerable loss of faith in repressive and punitive techniques, especially in dealing with juveniles.
4. There seems to be no strong negative reaction to the psychiatrist and he is coming to be regarded as the logical person to handle clearly identifiable cases of mental disorder. He is also beginning to be regarded as a useful resource in dealing with less serious personality problems, although here he is still handicapped because of a certain stigma that attaches to his subjects.
5. The lawyers represent a minor stronghold of reaction against psychiatry and against modern ideas of how to treat juvenile delinquency.

\* Paper presented at the American Sociological Association, September 1954.  
<sup>1</sup> Giles, "The Social View of Somatic Illness," *Journal of Somatic Medicine*, various issues, 1954.  
<sup>2</sup> Benj, "Student of Psychology," *Journal of Psychology*, 1954.



## GUIDED GROUP INTERACTION IN CORRECTIONAL WORK\*

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**S**TARTING with the work of Dr. Joseph H. Pratt at the turn of the century, group therapy developed slowly until the demand for psychotherapy by the armed forces during World War II exceeded the supply of trained personnel. The use of group methods to meet this demand for psychotherapy served as a stimulus to the group therapy movement and resulted in a large number of individuals developing programs in this area. Many of these persons had little or no previous experience with group therapy, and the activities that went on under its banner ranged all the way from meetings which combined inspirational exhortations with the techniques of commercial salesmanship to completely analytical sessions where group activity was directed toward the psychoanalysis of individual participants.<sup>1</sup> The interest developed in the application of these specialized group techniques persisted after the war, and descriptions of group-therapy programs are now commonplace.<sup>2</sup>

While a body of descriptive material has been developed in this general area and some progress has been made in the direction of scientific research, much remains to be accomplished. The literature frequently reveals vague definitions of central concepts, and as one reviewer observes, "... has been so confusing that one thinks there are as many forms of group therapy as there are practitioners." Most group-therapy pro-

grams have in common the gathering together of persons with problems in a more-or-less permissive environment where, with the leader, these problems are discussed and analyzed and group activity is directed toward helping participants modify old values or acquire new ones.

The use of group therapy in the treatment of prisoners was employed prior to World War II, but at the Fifth Service Command Rehabilitation Center, Fort Knox, Kentucky, where delinquent soldiers were processed to determine restoration to duty or further confinement at a disciplinary barracks, first extensive use was made of this specialized group method with persons in confinement.<sup>3</sup>

After the war, several civilian correctional institutions established programs of group therapy in an attempt to exploit the possibility of this method in their treatment programs. In New Jersey's correctional institutions, the application of this method is referred to as Guided Group Interaction.<sup>4</sup> The use of the phrase Guided Group Interaction was selected for a variety of reasons, but primarily to indicate that:

1. Not all prisoners are mentally abnormal or sick.
2. The leader, especially in initial sessions, assumes an active role as contrasted to his more passive role in other types of group therapy.

\*Paper read at the annual meeting of the American Sociological Society held in Denver, September 7-9, 1950.

<sup>1</sup>Giles Thomas, "Group Psychotherapy; A Review of the Most Recent Literature," *Psychosomatic Medicine*, 5 (April 1948), for a review of various methods according to emphasis of the therapist.

<sup>2</sup>Benjamin Kotkov, "A Bibliography for the Student of Group Therapy," *Journal of Clinical Psychology*, 6 (January 1950), includes 579 books, journal articles, etc.

<sup>3</sup>J. Abrahams and L. W. McCorkle, "Group Psychotherapy at an Army Rehabilitation Center," *Diseases of the Nervous System*, February, 1947; J. Abrahams and L. W. McCorkle, "Group Psychotherapy of Military Offenders," *American Journal of Sociology*, 51 (March, 1946), 455-464.

<sup>4</sup>F. L. Bixby and L. W. McCorkle, "A Recorded Presentation of a Program of Guided Group Interaction in New Jersey's Correctional Institutions," *Boston Proceedings of the 78th Annual Congress of Correction of American Prison Association*, Boston, 1948.

3. This type of group activity is distinguished from the more exhaustive type of analysis that is characteristic of group psychotherapy.

4. Modification takes place in the application of group-therapy principles when applied to the unique environment of the penal and correctional institution.

The following effort to present what seems to us important in a social-psychological analysis of guided group interaction is in the form of a series of observations that fit into our everyday experiences with these inmate groups. This presentation is made with the thought of stimulating increasing interest in this technique with its research needs among sociologists, who, because of their unique point of view, should make valuable theoretical and research contributions to the growing body of literature in this area.<sup>5</sup> It is, of course, impossible to present the completeness and rich complexity of the transactions that take place in a group session, and the material we have selected for presentation indicates not only our opinion that it will be useful in this analysis and representative of what can happen in correctional institutions but also our point of view. As Vold points out in a recent discussion of contemporary criminology, "Case histories of all sorts (psychiatric, psychological, and sociological) can be quoted to support almost any interpretation of personality characteristics that the investigator happens to prefer."<sup>6</sup> This same observation with equal accuracy can be made of guided group interaction sessions, and the writers would like to caution the readers about the danger of confusing the fragments of group sessions that are presented with any one session or the total of all such sessions.

After the suspicious and guarded responses of the first two or three meetings in which

the inmate's behavior conforms to his conception of the inmate's role, there is exploring and testing of what might be referred to as his therapeutic role in the guided group interaction sessions. During this period, the absence of established ways of relating to one another in this novel situation results in aimless and disorganized physical and verbal expression. Members are preoccupied with themselves and seem anxious to take advantage of their new freedom. In the following recording of a group of reformatory inmates, note the inability of this collection of individuals to take consistent action and the absence of any established pattern of response:

L—Now I want to say something.

O—Go ahead.

L—Look youngster, I never . . .

B—That's me. (Laughter and noise)

P—Look.

A—What's your name?

R—Walt.

P—Look, don't ever mention my name. I don't hit people on the head. I don't, and I don't intend to start. Understand me?

C—Understand me?

P—Understand? I talk to them, my voice does enough.

K—He's excited, excited.

P—No, I am not.

K—What? What?

Since participants must slowly learn the expectation of their role in therapy groups, the leader actively participates in the establishment of social rules for the members. He may accomplish this by a brief statement at the first meeting in which he defines the therapy situation. In subsequent meetings, he will have many opportunities to reinforce his earlier definition by indicating approval of certain types of behavior. In the following brief inspirational speech at the close of a session, the leader tries to secure the consensus of all the members to an approved way of behavior in the group meetings:

He put his problem down and he was honest about it. He wanted to tell us about it, and most important of all, he asked us to help him. He felt about us that we would like him without

<sup>5</sup> Marshall B. Clinard, "The Group Approach to Social Reintegration," *American Sociological Review*, 14 (April, 1949), 257-262.

<sup>6</sup> George B. Vold, "Criminology at Crossroads," paper read at meeting of *Midwestern Sociological Society*, Omaha, Nebraska, 1950.

any front. That's a very important thing, isn't it? When we can feel about the other guy that he's going to like us without the front. When I can feel about you that you will like me whether I have \$10,000 or \$10, then I feel good with you, don't I? So M—— must have felt a little better with us today. Just as he must have felt better with us, we must have all felt a little better because we started to be able to admit to one another and to ourselves that we had these shells and we have to look at these shells. And M—— has started to look at his shell, and as he goes on, he is going to find out some more things about himself, just as we go along, we're all going to be able to put them together. We can only go on if we feel people are going to like us without the shell. M—— can't go on unless he continues to feel we are going to like him without the shell.

During the hour session, a variety of methods, which range from the use of visual aids to interviewing techniques, are used to draw out each prisoner so that, in time, his values are exposed and reflected back to him. In this process, participants are afforded the opportunity to ventilate freely their feelings around significant objects. In this recording of a group of military prisoners, observe how the values of some are challenged by others, and how group reflection of the past behavior of members with reference to M.P.s replaces mass hostility toward the leader:

A—If somebody attacked you with a club, what would you do?

Leader—What do you think the fellow over here would do?

E—I'd beat 'im. Damn right I would!

Leader—All right, when somebody attacks you with a club, you attempt to defend yourself.

A—That's right. And yet and still I was charged with assault and battery and I didn't start anything with anyone. The guy comes up and swings at me with a club—it's lucky that he missed. And then I start defendin' myself and here comes the four chimin' in and startin' to shootin' at me.

B—Yeah!

O—Isn't there some regulation that they are not to pull that gun only in defense of your life?

P—Correct!

R—I'm talking about regulations, not records.

L—What's the matter with the M.P.s? Not a damn thing!

W—They're no damn good.

F—They're a bunch of P——s!

A—The M.P.s are supposed to conduct order, not go around looking for trouble.

H—That's right. The same thing happened to my buddy. An M.P. hit him right in the mouth and knocked a tooth out. Now, he could have hit him on the head; he didn't have to hit him on the mouth.

Leader—Why is it, I wonder, that one guy is always getting into trouble with the M.P.s? Other soldiers go right along and they never get into trouble.

K—It's the M.P.s.

P—It is not!

Q—It is, too!

R—It's not, by God!

S—It's the individual.

Leader—It's the individual. Well, what is it about these individuals then that they get into difficulties with the M.P.s? Some guys go along in the Army and never got into trouble with the M.P.s.

J—I'll tell you. Out in New York City you don't see the M.P.'s f—— with the soldiers because they are at a disadvantage. And every morning about three or four o'clock in the morning, you see some M.P. in an alley gettin' the s—— walloped out of him.

—It's all the same things.

—Damn right.

Leader—We still haven't gotten to the point. We just talked about M.P.s, but what is there about some people that they are always getting mixed up with the M.P.s?

S—They think the M.P. is the wrong type of guy. That's the g—— reason.

A—To a certain extent he is.

S—All of them? No, by God!

A—I wouldn't say all of them.

—Most of them are no g—— good.

G—When the M.P.s tell you to button up your pocket, why not just button it up and go on and forget about it instead of calling him a s—— and wanting to fight him?

As group sessions continue, participants evolve from their newly established relationships and roles a set of expectations for meetings that enable members to discuss their problems more completely. To develop this kind of group activity, which several

writers refer to as the "period of confiding" with the theme question "Why are you this way?", requires the utmost in skill and patience on the part of the leader.<sup>7</sup> It is difficult for inmates to give up their habitual ways of relating to one another and reveal their private worlds to their peers. In the following, observe the difficulty one inmate has when, for the first time, he relates events from his past to make his present problems understandable to the group:

M—See, when I was a kid I put a lot of trust in my mother and she died when I was a kid and I switched to my father but he was sent to the State Prison and I didn't have anyone to fall back on. So the only way for me to get along in life was to put up a flashy front and spend my time playing horses, gambling, and running around with women. I did this, more or less, to cover up this feeling of being by myself and not being able to depend on anyone.

As this process continues, the non-uniqueness of problems becomes a source of security to all and members find increased satisfactions in their roles in the group sessions.

Leader—When you didn't like a job, you quit. All right, now that we got that, let's try to figure this fellow out. Now what did you want to say, fellow?

P—The same thing.

Leader—The same thing? You wanted to say the same thing? You did a similar thing to him?

P—Practically in every respect.

The leader can actively participate in this process.

B—Well, I believe that a person in that particular instance has had some incident happen to him that took that confidence away from him. Perhaps at one time he was deeply attached to a female, had a very deep friendship with someone, and something happened to frustrate that friendship or affection—some incident.

Leader—Then he's unable to have it any more.

B—He invested so much in that particular one . . . Well, I mean I could say I was speak-

ing from experience. He invested so much in that particular person, rather, that when they upset that friendship or that affection he just lost confidence in everybody else.

Leader—Everybody shakes their head "yes" on that. Nobody seems to disagree. You are quite emphatic in shaking your head. Why? Why do you feel so strongly that's true?

K—Same thing has happened to me.

Leader—The same thing has happened to you in your life.

But it is more useful when it spontaneously comes from members.

F—Well, I mean when he said there, like Bob said, he should be able to get along with people, be able to get himself accustomed to different people—to associate with them and go along with them, not to be kinda shy or afraid of them in any way, and if he wants to be by himself then he is okay. Well, like myself I like to be alone but I can be in a crowd of people and get along as well with a hundred as I can with one, but I'd rather be by myself altogether.

In time, group members develop for one another what Cooley described as the primary-group feelings of sentimental regard as against indifference or hostility. In the following group, attending their sixth session, observe the lack of friendliness or concern for S- in J-'s fairly accurate remarks on his behavior in the wings:

S—Well, I might. But he wants me to adjust myself to the people in A Wing and learn to get along in A Wing.

J—Why can't you get along in A Wing?

S—Because I can't.

J—What makes you think you can get along in another wing?

S—Because I'd be by myself then.

A—Can't you be by yourself in A Wing?

S—No.

J—Why can't you make those fellows leave you alone? You want to stay by yourself.

S—That is not the point. The point is they'll turn around and bother you anyhow. At least in one of the lock-up wings, if you don't want nobody around, you go in your cell and lock the door, and the hell with them. Right?

O—Sure.

B—Do you mean to tell me people bother you, S—?

S—Yeah.

<sup>7</sup> Hubert Coffey, Mervin Freedman, Timothy Leary, Abel Ossorio, "Community Service and Social Research—Group Psychotherapy in a Church Program," *Journal of Social Issues*, 6 (1950).



J—I think S—— bothers people if I know S——. I locked with you for three and a half months. If you come over to E-2 and pull the shit you pulled on B-3.

S—Well, anyhow, that's what he said. I am just stating what he said, that's all.

Leader—Well, I think S—— made a point. He said that really what determines whether or not a guy is ready to go out depends on his ability to get along in any kind of situation.

In the next excerpt of a group who have attended twenty-four sessions prior to this recording, note the sympathy and understanding communicated to F— by K—. F— has just finished telling the group of his feeling of responsibility for the death of an older brother:

Leader—He says I still feel it. Well, what about that?

I—Well, I mean he feels that he was responsible for it himself and he is doing a lot of things that ordinarily wouldn't come naturally that he wouldn't do.

Leader—Yes.

K—Well, I think the thing for him to do when he is by himself—kind of feel himself out, think back and figure out just how far is he carrying it—is he carrying it too far—is he taking everything to blame on himself. Spread it out and find out that some things he could be responsible for that he could probably have prevented and some things that he couldn't. Carry that feeling that far and maybe he would get rid of some of it anyway and go along like that, maybe he would feel better.

Leader—You think he might lose some of it in this group today?

K—Yes, he might.

In the next recording, one inmate at the close of the session sums up his effort to take over the role of the other boys and look at his behavior in the institution. While his peers are not impressed with his statement of good intentions, he re-defines his promise to change so that it is more acceptable to the group:

Q—C——, I don't think you try to brown-nose, but you have a bad habit of trying to take the front, to take the spotlight. No matter what comes up, C—— wants to be up in front.

E—What actually has happened, C—— takes

group therapy too seriously; uh, uh, he knows he is doing wrong and he too readily admits he is doing wrong instead of arguing it out with himself. All he says—we tell him something and he just turns it around and admits he is doing wrong, but actually he isn't thinking it over. He just takes our word and just looks it over and thinks it is right and says he is going to do right. But I'll bet you this afternoon he will be out there f—— around just like everybody else.

C—You know, I have never saw myself in this same light before. This is the first time I have ever been broken down actually and it changed my opinion of myself. You know what I mean. I never actually saw myself in this same light.

R—In ten minutes?

C—In ten minutes, yes.

S—By change, what he means about himself now is that he is seeing himself differently since he went through this.

C—That's right!

S—And he got some kind of different opinion. He don't mean he's changing right now but along the road down the deal he is continuing to try to change. You know what I mean—gradually. Every day he'll say, "Well, I'm doing something wrong since the boys down there gave me the once-over." He means he'll gradually change but he don't mean just jump up and change over at once. He means he is seeing himself different and he is going to try to come up to it. Raise up to it.

R—C—— has something he's going to have to get out of his mind, and it's not easy to get out of his mind overnight, and that's the fact that he thinks other men aren't as good as him. He thinks he's better than other men. He's got that in the back of his head. Even that guitar he plays he thinks he can play better than anybody else. He has got to get it out of his head.

Guided group interaction is offered not as a panacea for the ills of contemporary penology, merely as a useful device worthy of experimentation and research. Our experience leads us to believe it is a helpful technique in the development of programs which attempt to utilize existing resources in an effort to develop good citizens instead of habitual offenders in our correctional institutions.

## DISCUSSION

GEORGE B. VOLD

*University of Minnesota*

The Bixby-McCorkle paper is essentially non-controversial, being a report on some of the thinking that lies back of an experiment in progress. It is presented with due humility and carefully qualified as to implications and conclusions. I can only express hearty agreement with the concluding paragraph. It is a model of restraint.

The New Jersey project in Guided Group Interaction is one with which I have had some contact and one that I heartily endorse. Mr. McCorkle's work in New Jersey was the subject of a very excellent article in the *Reader's Digest*.<sup>1</sup> He was also the recipient of that periodical's award for the best work being done in the field of rehabilitation of delinquents.

My comments will be directed to some matters of basic orientation and underlying research methodology that are involved in this work. Most research in criminology makes use of one or another of two different and to a considerable extent conflicting approaches to the problem of crime causation. One approach says in effect that the individuals are delinquent because there is something wrong with them "inside themselves" as individuals—straighten out this difficulty and the major purpose of rehabilitation will have been accomplished. The other approach operates from the basic, implicit assumption that in a delinquency area delinquency is the normal response of the normal individual—that the non-delinquent is really the "problem case," the non-conformist whose behavior needs to be accounted for. Guided Group Interaction quite explicitly assumes the first or the individualistic approach—straighten out the "mixed-up inside" problems of the individuals concerned, get them "out of their shells" and the essential step in their rehabilitation will have been taken.

A fundamental difficulty of this method lies in the danger of confusing the effect of the strong personality of the leader with the method

or technique that the leader talks about. Many "experiments" in penology in the past have been notably tied to the personality of the man who made them worthy of note. Keep in mind, for example, the exciting experiments in administration and attempts at rehabilitation associated with the names of Obermaier, Montesinos, Osborne, Plummer, and Gill. Each accomplished something of note, for a time, but in every case the experiment depended for success on the imprint of a particular personality on a group of inmates. Change of leaders, or change of inmates, through too rapid transfers, and the initial flush of success fades and the institution returns to conventional routine. In every case one of the important ingredients responsible for failure was the simple psychological fact that it is impossible to institutionalize friendship, or to channel it into administrative segments so that the leader and the inmate automatically and as per convenient schedule become friends and trust one another with the deeper facets of personality make-up. This is the paradox of wholesale "individualization" of treatment!

The experiment in Guided Group Interaction must face two basic, critical questions:

1. Is it a technique that can be administered by ordinary individuals after suitable training, or is it a non-specific influence that depends for success on a particular personality?
2. Is it a technique that shows lasting effects in careful comparison with non-guided "control groups" carefully matched for accurate comparison?

If it is special kind of personality, either of leader or of inmate, that makes for success, then there is need to devise techniques for discovering and selecting such personalities, both for the leadership positions and in finding inmates for the group to be "guided."

From the standpoint of research methodology, the inclusion of transcriptions of actual conversations in progress during Guided Group Interaction sessions raises the fundamental prob-

<sup>1</sup> James Finan, "Inside the Prison," *Reader's Digest*, May 1950, pp. 61-72.

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lem of the selection of materials. Have the responses been selected to illustrate what the leader wants to illustrate? Could other assortings of responses be made to illustrate something else—perhaps the futility of “guided group conversation,” as the method also has been called? Research on how to make use of transcriptions seems urgently needed. Perhaps investigators with different philosophical and methodological orientations could be induced to listen to, and to interpret, a succession of extensive transcriptions of actual group sessions. It would be interesting to see to what

extent they reach similar conclusions from the babel of conversation!

Some of these questions are directly involved in the further research now under way in connection with the New Jersey project in Guided Group Interaction. It would be my judgment that this experiment constitutes some of the most important research in criminology now going on in the United States. This work is likely to produce results of basic importance both for the practice of penology and for the advancement of research methodology in the field.

## CHANNELS OF COMMUNICATION IN SMALL GROUPS\*

ROBERT F. BÁLES, FRED L. STRODTBECK, THEODORE M. MILLS, AND  
MARY E. ROSEBOROUGH†

*Harvard University*

THE frequencies of communication between members participating in small face-to-face groups show certain striking regularities which have not previously been described. The observations reported here were made over a period of several years. Various sorts of small problem-solving groups were observed with a variety of primary hypotheses in mind. The method of analyzing the data reported here had not been developed at that time. The possibility of using the data for the present investigation grew from the fact that a uniform method of observation was used throughout.

A number of the groups were composed of students brought together in an experimental room to solve various contrived problems. Others were non-student committees or work groups in their natural settings. Two therapy groups through a series of

sessions were included. A large number of groups were case-discussion meetings of diagnostic councils operating in a research-clinic setting. It is probably accurate to say that most of the groups could be called decision-making, or problem-solving, or discussion groups.

The groups ranged in size from three to ten persons. The conversation generally proceeded so that one person talked at a time, and all members in the particular group were attending the same conversation. In this sense, these groups might be said to have a “single focus,” that is, they did not involve a number of conversations proceeding at the same time, as one finds at a cocktail party or in a hotel lobby. The single focus is probably a limiting condition of fundamental importance in the generalizations reported here. Larger groups or other types of groups with multiple foci are probably somewhat different with regard to certain aspects of channel frequency.

\*The substance of the paper was presented to the annual meeting of the American Sociological Society, held in Denver, September 7-9, 1950.

†The research reported in this paper was facilitated by the Laboratory of Social Relations, Harvard University. The second-named author is now at Yale University. Frederick Mosteller, John Tukey, and Christoph Heinicke have given helpful suggestions concerning the analysis of the data.

### HOW THE OBSERVATIONS WERE COLLECTED

Each act in the inter-communication process is recorded.<sup>1</sup> If the act is verbal the unit

<sup>1</sup> For a more complete description of the method, see Robert F. Bales, *Interaction Process Analysis*:

is usually the simple subject-predicate combination. If the act is non-verbal, the unit is the smallest overt segment of behavior that has "meaning" to others in the group. With this method of recording, the number of units will be many for a lengthy speech given by one member of the group, and a laugh, a nod, or a fidget will each constitute a unit. We have found that the number of scores recorded for a given person bears a close direct relation to the total time he consumes in his overt participation. Thus it is quite probable that the relations we describe can be duplicated in terms of time-units alone. Several observation systems which have qualitative categories quite different from the system employed by the present investigators still produce information concerning who speaks, to whom he speaks, and the number of acts—the essential data involved in the present report. Our findings can probably be reproduced by other methods and checked by other investigators.

The originator of each act, as it occurs, is recorded. At the same time the recipient of the act or the "communication target" is recorded. For example, an act directed from person one to person two is recorded: 1-2. For convenience, a number is assigned to each member of the group, and the symbol 0 is used to designate the group as a whole without distinction as to separate persons. Any member of the group may be an originator or a target of a particular act, but the group as a whole may be only a target. The relation between any two members (including both directions, e.g. 1-2, 2-1) or between any member and the group as a whole (e.g. 1-0) we call a "channel of communication."

#### HOW THE OBSERVATIONS WERE ORDERED

When originators are placed so as to designate a series of rows of a matrix, and the

*A Method for the Study of Small Groups*, Cambridge, Massachusetts: Addison-Wesley Press, 1950; also, "A Set of Categories for the Analysis of Small Group Interaction," *American Sociological Review*, 15 (April, 1950); and Robert F. Bales and Fred L. Strodbeck, "Phases in Group Problem Solving," to appear in *Journal of Abnormal and Social Psychology*, July or October, 1951.

possible targets are placed to designate a series of columns, a matrix with  $n$  rows and  $n+1$  columns results, where  $n$ =number of members in the group. The tabulation matrix for a three-man group is illustrated below:

Originator	Target			Total Initiated
	1	2	3	
1	—	1-2	1-3	1-0
2	2-1	—	2-3	2-0
3	3-1	3-2	—	3-0
Total Received				

The first ordering operation consists of tallying entries as scored by the observer in the appropriate cell in the tabulation matrix and finding the totals for each row and column.

The second ordering operation consists of ranking the members according to the frequency of their respective originations as indicated in the row totals for acts initiated. The person originating the greatest total number of acts is placed in the first row; the person originating the second largest total is placed in the second row, and so on for  $n$  members. The columns are then arranged so that the number of the row of an originator corresponds to his column number as target. In this operation the arrangement of columns is made regardless of the frequency of acts received. The identifying numbers of the members are then changed, so that 1 now represents the member initiating the highest total number of acts, 2 the second highest, and so on. We call the position obtained by this operation the *basic initiating rank* for each individual. A matrix ordered according to the basic initiating ranks of the members is called an *ordered interaction matrix*.

We prepared an ordered interaction matrix for each separate group or session observed. Then one further operation was performed on the collection of ordered matrices for groups of each size. Corresponding cells in each group were combined, that is, cell 1-2 for group 1 was added to cell 1-2 for group 2, group 3, . . .  $n$ ; cell 1-3 for group 1 was added to cell 1-3 for group 2, . . .  $n$ , and so on for all cells. This addition resulted in a

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## CHANNELS OF COMMUNICATION IN SMALL GROUPS

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TABLE 1. AGGREGATE MATRIX FOR 18 SESSIONS OF SIX-MAN GROUPS

Person Originating Act	To Individuals						Total to Indi- viduals	To Group As a Whole	Total Initiated
	1	2	3	4	5	6		0	
1		1238	961	545	445	317	3506	5661	9167
2	1748		443	310	175	102	2778	1211	3989
3	1371	415		305	125	69	2285	742	3027
4	952	310	282		83	49	1676	676	2352
5	662	224	144	83		28	1141	443	1584
6	470	126	114	65	44		819	373	1192
Total Received	5203	2313	1944	1308	872	565	12205	9106	21311

single composite matrix for each size of group. A matrix formed by the addition of separate group matrices in this way we call an *aggregate interaction matrix*.

## REPORT OF FINDINGS

The results of these operations for the 18 six-man groups in our sample are presented in Table 1. We use this aggregate matrix for illustration because it perfectly exemplifies the generalizations we wish to state. In this aggregate matrix it will be seen that each row and each column presents a rank order which is the same as the basic initiating rank of the members. These are the essential uniformities we now wish to describe in more detail. In order to have a more exact notation for the statement of our findings, we present in Table 2 an ordered matrix paradigm suitable for groups of three or more persons with a generalized symbolic designation for each of the cells.

We may describe the basic ranking operation performed on each separate group in

terms of the generalized notation in Table 2, and illustrate by concrete example from Table 1.

$$(0) \sum a_{1j} + a_{10} > \sum a_{2j} + a_{20} > \dots > \sum a_{nj} + a_{n0} \\ i \leq j \leq n; j \neq i$$

Example (0):

$$9167 > 3989 > 3027 > 2352 > 1584 > 1192$$

This ranking operation when performed on data for a single group provides the basic initiating rank for each individual. For convenience we shall speak of rank 1 as the "highest" rank of a series. When data for a given group are ranked in this manner, the following generalizations tend to hold.

(1) *Each row (acts directed by one individual to persons other than himself) tends to be ordered so that the cell for the person of the highest basic initiating rank receives the largest number, the cell for the person of the second highest basic initiating rank receives the second largest, and so forth.*

This generalization is somewhat awkward to communicate accurately in words, because

TABLE 2. THE PARADIGM FOR THE ORDERED MATRIX

Person Originating Act	To Individuals				Total to Individuals	To Group As A Whole	Total Initiated
	1	2	...	n			
1	—	$a_{12}$	...	$a_{1n}$	$\sum a_{1j}$	$a_{10}$	$\sum a_{1j} + a_{10}$
2	$a_{21}$	—	...	$a_{2n}$	$\sum a_{2j}$	$a_{20}$	$\sum a_{2j} + a_{20}$
...	...	...	—	...	...	...	...
n	$a_{n1}$	$a_{n2}$	...	—	$\sum a_{nj}$	$a_{n0}$	$\sum a_{nj} + a_{n0}$
Total Received	$\sum a_{i1}$	$\sum a_{i2}$	...	$\sum a_{in}$	$\sum \sum a_{ij}$	$\sum a_{i0}$	$\sum \sum a_{ij} + a_{i0}$

\* All summations from  $i, j=1$  to  $i, j=n, j \neq i$ .

all individuals in the group have a basic initiating rank, and all are included in the target columns, yet the row of acts directed by any given person contains one empty cell—the cell indicating the given person as the target of his own acts. The generalization is thus understood with the restriction that the cell indicating the person originating the acts as target of his own acts is excluded. In the symbolic statement of the generalization, below, this difficulty is unequivocally resolved by the condition,  $i \neq j$ .

$$(1) a_{i1} > a_{i2} > \dots > a_{in} \quad 1 \leq i \leq n; i \neq j$$

Example (1):

$$\begin{array}{c} 1238 > 961 > 545 > 445 > 317 \\ 1748 > 443 > 310 > 175 > 102 \\ \vdots \\ 470 > 126 > 114 > 65 > 44 \end{array}$$

Since the *sums* of acts directed to participants tend to reflect the relationships described for each row, we may say somewhat more generally:

(1a) *The rank of the total number of acts received by an individual tends to correspond to his basic initiating rank.*

$$(1a) \Sigma a_{i1} > \Sigma a_{i2} > \dots > \Sigma a_{in} \quad 1 \leq i \leq n; i \neq j$$

Example (1a):

$$5203 > 2313 > 1944 > 1308 > 872 > 565$$

(2) *Each column (acts received by one individual from persons other than himself) tends to be ordered so that the values correspond to the basic initiating ranks of persons originating the acts.* Here again it is understood that the column representing acts received by the  $j^{\text{th}}$  person omits the cell indicating this person as an initiator.

$$(2) a_{1j} > a_{2j} > \dots > a_{nj} \quad 1 \leq j \leq n; i \neq j$$

Example (2):

$$\begin{array}{c} 1748 > 1371 > 952 > 662 > 470 \\ 1238 > 415 > 310 > 224 > 126 \\ \vdots \\ 317 > 102 > 69 > 49 > 28 \end{array}$$

Again there is a summary statement which may be made in terms of totals:

(2a) *The rank of the number of acts directed by an originator to all other specific*

*individuals tends to be ordered to correspond to the basic initiating rank of the originator.*

$$(2a) \Sigma a_{1j} > \Sigma a_{2j} > \dots > \Sigma a_{nj} \quad 1 \leq j \leq n; i \neq j$$

Example (2a):

$$3506 > 2778 > 2285 > 1676 > 1141 > 819$$

Finally, (3) *The rank of the number of acts directed by an individual to the group as a whole tends to correspond to his basic initiating rank.*

$$(3) a_{i0} > a_{20} > \dots > a_{n0}$$

Example (3):

$$5661 > 1211 > 742 > 676 > 443 > 373$$

With regard to generalizations (2a) and (3), it should be noted that even though the basic initiating rank is determined by the sum of acts directed to specific individuals and to the group as a whole, we have nevertheless gained an increment of precision in description by specifying that *each* of these columns tends to be ordered as the basic initiating rank.

#### HOW WELL DO THE GENERALIZATIONS REPRESENT THE DATA?

Our generalizations have been illustrated thus far with aggregate data for six-man groups. We now turn to the presentation of data which will illustrate the applicability of the generalizations (a) for the range of groups from three to ten persons, and (b) for individual sessions as well as for aggregates of individual sessions.

Our sample consists of 171 sessions in which approximately 138,000 acts were observed.<sup>2</sup> The writers have included *all* cases available to them which have been observed by the uniform technique, but since the

<sup>2</sup> The writers wish to express their appreciation to the following persons for acting as observers or contributing data from studies of their own in which the method was used: John Evans, data from two Veterans Administration groups of 16 and 12 sessions each; Harry Levin, some 90 four-man group sessions from the University of Michigan; and Janice Berg, Nathan Kogan, Robert Dowd, Derm Barrett, Jack Finger, Irving Rosow, and Howard Smith, data on three-man groups collected in connection with work in a Harvard Seminar.

observations were carried out in terms of research designs which were quite independent of this paper, the cases are very unevenly distributed in terms of group size. For example only one nine- and three ten-man group sessions are available for analysis.

Each of the generalizations has been stated as a proposition predicting an expected order 1, 2, . . . n. It is desirable to compare the agreement between observed and expected for each of these propositions by a parallel technique. In order to express the correspondence in a uniform manner notwithstanding the variation in the size of the group, we define "d" as the difference between the expected and the observed rank. It may be shown that if all orders are equally likely

$$\bar{d}^2 = \frac{(n^3 - n)}{6}$$

and

$$\sigma_{\bar{d}^2} = \sqrt{\frac{n^2 (n+1)^2 (n-1)}{N \cdot 36}}$$

In the tests we shall report we establish the ratio

$$\frac{\bar{d}_e^2 - \bar{d}_o^2}{\sigma_{\bar{d}^2}}$$

and make a one-sided test of the null hypothesis.

$$\bar{d}_o^2 \leq \bar{d}_e^2$$

The subscripts e and o refer to "expected" and "observed" in the formulae above.

When for any particular test we obtain a probability less than .05 we reject the null hypothesis and conclude that there is a significant departure from randomness in the direction of the ordering that we have predicted. For any particular hypothesis and for each group size two tests are made: one on the aggregate matrix and one on the full array of individual sessions.

In Table 3 we present the eighty probabilities derived from a systematic application of this test over the range of group sizes and hypotheses. The evidence overwhelmingly supports the generalizations

presented. In all cases the probabilities observed are either less than .01, or the minimum probability theoretically attainable.<sup>3</sup>

This degree of conformity with expectation is sufficiently unequivocal to lead us to believe that if we had done our basic ranking in terms of "acts received," "acts to individuals," or "acts to group as a whole" instead of "total acts initiated," substantially similar results would have been attained.

#### DISCUSSION

We believe that the detection of these regularities represents a significant gain in our knowledge about the distribution of communications in small groups, and provides a basic framework of order within which many more detailed analyses of the interaction process may be made. We have noted other apparently typical features of the matrix which are probably sociologically significant, although they cannot be described systematically here. A few features can be illustrated, however, by reference to Table 1.

For example, it can be seen that the top man addresses considerably more remarks to the group as a whole than he addresses to specific individuals (5661 > 3506). All men of lower basic initiating rank address more of their remarks to specific individuals (and markedly more to the top man) than to the group as a whole (2778 > 1211, etc.). This seems to indicate that the top man is acting as a kind of communications center, and in this sense is performing a leadership function.

When the amounts each man gives out to the sum of specific individuals is compared to the amounts he receives, a still more suggestive picture is obtained. The top man receives more from particular others in total than he gives out to them specifically

<sup>3</sup> An example where the probability shown is above .01, but still a minimum, occurs in the row for 3-person groups, aggregate data. Our aggregate matrix for 3-person groups exactly conforms to all five hypotheses, but since the n is small, three of the applicable probabilities are above .05.

TABLE 3. SIGNIFICANCE LEVELS BY SIZE OF GROUP AND HYPOTHESIS

Size of Group	Number of Sessions	Number of Acts	Hypothesis				
			1	1a	2	2a	3
3	26	9,304	.000*	.000	.000	.000	.000
			.042	.079	.042	.079	.079
4	89	58,218	.000	.000	.000	.000	.000
			.002	.042	.002	.042	.042
5	9	10,714	.000	.000	.000	.000	.000
			.000	.036	.000	.036	.036
6	18	21,311	.000	.000	.000	.000	.000
			.000	.013	.000	.013	.013
7	15	22,044	.000	.000	.000	.000	.000
			.000	.007	.000	.007	.007
8	10	12,830	.000	.000	.000	.000	.000
			.000	.004	.000	.004	.006
9	1	1,422	.000	.008	.000	.002	.002
			.000	.008	.000	.002	.002
10	3	2,823	.000	.000	.000	.000	.000
			.000	.002	.000	.001	.009

\* Cell values above represent the probability of the null hypothesis of random ordering. The non-italicized values refer to the array of individual ordered matrices, and the italicized values refer to the ordered aggregate matrices.

(5203 > 3506). His contributions tend to be addressed more frequently to the group as a whole than to the specific persons who address him. All other men, however, tend to receive less from particular others than they give out to them (e.g., for Rank 6 man, 565 < 819).

In our exploratory analyses of data we have constructed matrices for particular categories of activity, such as agreement, disagreement, asking for information, etc., as defined in the system of observational categories we use. The matrices are constructed according to the basic initiating rank of the individuals (based on totals of all types of activity) as the ordering principle. Matrices of these kinds which have been examined so far tend to show the top man giving out more information and opinion to specific individuals than he receives, while, on the contrary, low men give out more agreement, disagreement, and requests for information than they receive. The analysis of the quality of the communications in terms of general theory regarding the properties of social systems appears at present to be the most promising approach to the understanding and rationali-

zation of the generalizations we present here. It is expected that an approach of this kind will provide a fundamental basis for accounting for differences between matrices and exceptions to the generalizations we have made, as well as the typical features of the matrices.

The relative magnitudes of the number of acts falling in each cell of the ordered matrix can probably be expressed and predicted with much greater precision than a rank order description permits. This will require the development of mathematical models, but there is some doubt that appropriate models will be simple. One model often suggested for data like our series of rank orders of total acts initiated is the harmonic distribution. We are inclined to believe that no simple mathematical function of this type, which does not at least include more parameters than the size of the group, will really fit our data. However, because of the general interest in this model, we compare its predictions with our data below.

To obtain expected values in terms of the harmonic distribution we have employed the following formula:

\* The not present. As individual presented.



$$X_{(i, n)} = \frac{S}{\sum_{j=1}^n \frac{1}{j}}$$

where,

$X_{(i, n)}$  is the expected number of acts by the person with the  $i^{\text{th}}$  rank in a group of  $n$  persons, and

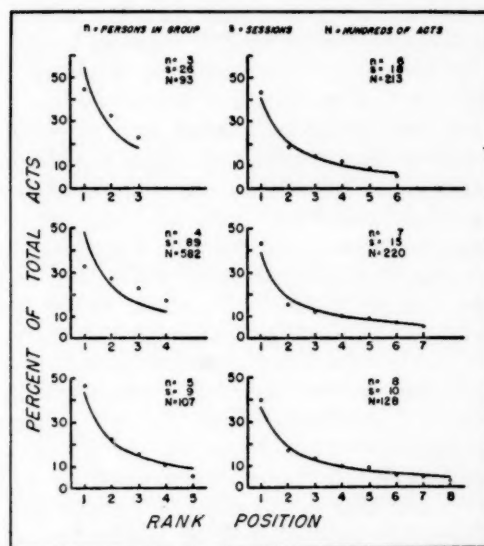
$S$  is the sum of acts in the set of data under consideration

In Chart 1 we have plotted a series of curves derived from the harmonic distribution model. Shown against the curves are a series of points representing data drawn from the "total initiated" column of our aggregate matrices.<sup>4</sup> It may be seen by inspection that the empirical curves for the groups under size 5 are flatter than expected, while those for size 5 and above are steeper. The top man in the larger group sizes initiates more activity than predicted by the model. Our groups of size 4 show a particularly poor fit. A large number of homogeneous sessions are included here, of groups working on a particular type of problem (making series of clinical ratings of personalities from case material). There was a formal allocation of reporting and recording roles, and systematic rotation of members between the roles. It seems reasonable to believe that conditions of this kind could affect the participation gradients in an atypical way.

The fit for groups of size 6 is the best obtained by this model. The observed data for their aggregate matrix have previously been presented in Table 1. The deviations between observed and expected values are presented in Table 4. In terms of chi square we may quickly determine that the fit is not sufficiently good to permit us to believe that the deviations have arisen as random fluctuation. With values of this magnitude, the chi-square criterion would require correspondence between the first two places of each value. By similar tests it can be shown that none of the fits is acceptable.

<sup>4</sup> The data for groups of sizes nine and ten are not presented because we have so few observations. As individual cases they are similar to those presented.

CHART 1. Rank Ordered Series of Total Acts Initiated, Compared with Harmonic Distribution, for Groups of Sizes Three to Eight



In assessing the importance of the findings reported in this paper there is one point which deserves great emphasis. The generalizations presented are average empirical tendencies. They are assumed to be the result of certain general conditions which we are not yet able to specify. But we assume that as conditions vary, the matrices will vary. Any specific group, or some particular types of groups, may present exceptions to the generalizations we describe, in one or more particulars, depending on

TABLE 4. NUMBER OF ACTS, BY RANK POSITION, FOR AGGREGATE OF 18 SIX-MAN GROUPS COMPARED WITH EXPECTED NUMBER OF ACTS BY HARMONIC ASSUMPTION

Rank Position	Theoretical Values	Expected*	Observed
1	.4082	8698	9167
2	.2041	4349	3989
3	.1361	2900	3027
4	.1020	2174	2352
5	.0816	1740	1584
6	.0680	1450	1192
Total	1.0000	21311	21311

\* Expected values represent the product of the total acts observed times the theoretical values.

the conditions operating. For example, we have often found particular exceptions to predicted rank positions in cases where one of the members disagrees with the others persistently, and so tends to attract or receive a disproportionate amount of communication. And we have also found exceptions when two highly interactive and agreeing members form a sub-group vis-a-vis a third neglected or rejected member. The average empirical tendencies described constitute a base-line expectation for the detection of discrepancies or exceptions which we believe will be useful in diagnostic and comparative analysis of particular groups. There are many sorts of experimental predictions that can now be made with an increased degree of accuracy through the use of the ordered matrix. The importance of the generalizations will be much increased as we are better able to identify and measure the effects of various kinds of conditions on them.

#### SUMMARY

The findings reported indicate that if participants in a small group are ranked

by the total number of acts they initiate, they will also tend to be ranked:

- (1) by the number of acts they receive,
- (2) by the number of acts they address to specific other individuals, and
- (3) by the number of acts they address to the group as a whole.

Exceptions to these general tendencies are expected to be diagnostic of special features of the social relationships in the group, of interest in the comparison of a given group with others.

More refined ways of analyzing the interaction matrix promise to yield further generalizations. The harmonic distribution model of total amounts of participation is found to be a very rough approximation, but ~~probably too simple~~. However, mathematical models taking into account more variables may be possible. In general, the most promising approach to an understanding of the matrix regularities seems to be through a sociological analysis of the nature of small social systems. This approach does not preclude, but rather invites formulation in terms of mathematical or statistical models.

## HUSBAND-WIFE INTERACTION OVER REVEALED DIFFERENCES\*

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**I**N the course of a series of pilot studies of power, or influence, in small group situations the writer has developed a procedure, called the revealed difference technique, which has shown promise in a first application to husband-wife interaction. In the attempt to validate the results ob-

tained by this technique, use has been made of similar groups in different cultures. The following paper is organized in a form to emphasize how this methodological innovation and the technique itself grew from successive sequences in which pilot findings led to further research operations.

*Background.* During 1948-49 the writer observed a series of groups engaged in decision-making. An effort was made to determine some of the correlates of differential ability to persuade others in accordance with the actor's desires. In one instance, four mathematics students were requested to recommend jointly the best of three pos-

\* Data for this paper were collected under the auspices of the Comparative Study of Values Project being conducted by the Social Relations Laboratory, Harvard University, with the assistance of the Rockefeller Foundation and the Peabody Museum. This paper was read at the annual meeting of the Eastern Sociological Society held in Boston, April, 1950.

sible solutions to particular problems. While these students were in the process of developing consensus they were asked to record privately the alternative they personally favored. Thus, the experimenter was provided with a continuous means of relating a type of private opinion to public behavior. The experimentation indicated that the ultimate decision could be most accurately predicted by simply weighting the privately pre-determined opinion of each participant by the total time he had spoken during the experimental interaction. This finding was duplicated in various groups who worked at the task of jointly selecting the best move in a chess problem. This simple answer did little, however, to recapture the subtlety and complexity of social interaction as it is generally understood.

We recognized that we had up to this time worked with *ad hoc* groups which had no group structure at the beginning of the observation period and no expectation of participating with one another at a later time. The problems they had considered were delimited and specific; the nature of their arguments and responses was highly structured. On the basis of this analysis, we were led to consider experimentation with groups whose members approached the opposite extreme of broad common interests, daily contact, and permanence—so-called primary groups.

Among the various types of primary groups that might profitably be studied, husband-wife dyads were selected because of the ease of replication of these units. Each couple was asked to pick three reference families with whom they were well acquainted. The husband and wife were then separated and requested to designate which of the three reference families most satisfactorily fulfilled a series of 26 conditions such as: Which family has the happiest children? Which family is the most religious? Which family is most ambitious? After both husband and wife had individually marked their choices they were requested to reconcile their differences and indicate a final "best" choice from the standpoint of their family. For the first ten

couples studied, this pooling took place with the experimenter out of the room and under conditions such that the couple did not know they were being observed or having their voices recorded. Their lack of knowledge of the observation was ascertained after the session, at which time their permission to use the material in a scientific inquiry was obtained.<sup>1</sup> The anticipated experimental difficulties—(a) producing "polite" interaction because of the intrusion of the experimenter, and (b) structuring the task to such a degree that the mode of interaction would be highly determined—were judged to have been satisfactorily avoided.

Omitting, for present purposes, a discussion of the content of the recorded protocols, it was found that women won 47 of the contested decisions and men, 36. In six of the eight cases in which there was a difference both in number of decisions-won and in talking-time, the spouse who talked most won the majority of the decisions. At this time there was no basis for appraising whether the women had won slightly more decisions because they had known more about the types of information under discussion, or whether the decision winning represented, as we had hoped, the operation of structured power relations in an area in which both participants were equally informed. The observed margin by which the women exceeded the men was not significant—a result which might have been much more valuable if we had predicted it in terms of independent knowledge of the equalitarian characteristics of the married veteran couples used in the sample. In short, further application was necessary to determine whether the technique was a valid method of indicating in any more general sense the balance of power between participants.

A field study was designed to throw further light on this problem. Three communities were selected which presumably differed in terms of the degree to which the wife was favored by the cultural phras-

<sup>1</sup> Ursula Marsh, Donald Michael, Theodore M. Mills, and Herbert Shepard were joint participants in this phase of the research.



ing of power. The communities were at the same time sufficiently small to increase greatly the probability that both spouses would be adequately, if not equally, informed concerning the behavior of the reference couples. The technique as described above was applied to ten couples from each of these cultures. It was proposed that the conformity of the experimental results with the a priori cultural expectations be taken as a crude measure of the validity with which the technique reflected power differences.

*Description of Cultures.* The cultures which were selected for study are geographically adjacent communities in the Arizona-New Mexico area. Briefly described, the groups are Navaho Indians; dry farmers from Texas who have recently homesteaded in the area; and early settlers who utilize a dam operated under the supervision of the Mormon church. These communities will be described in detail in forthcoming publications of the Comparative Study of Values Project.<sup>2</sup> For present purposes the communities will be designated Navaho, Texan, and Mormon. A brief recapitulation of power attributes of the culturally legitimized role of women in each culture is presented below.

The young Navaho man, who marries a girl from a moderately successful family, typically leaves his own family and resides with the girl's family and works under her father's direction until he has established himself as a responsible person. When this change of residence is made, the man leaves his sheep with his own family of orientation and his work activities result in little immediate increase in his own holdings. The children are considered a part of the wife's consanguine group, and marriages are generally unstable. Both men and women own sheep, but the women do the processing of wool into rugs and blankets. This assures the women a regular income throughout the year. The man has greater earning power

when he performs wage work, but the wage work opportunities are scarce and seasonal. The man is considered the head of the household, but the relative economic independence of the wife and her close integration with her own consanguine group effectively limit his exercise of power. All but one of the ten Navaho couples studied maintained Navaho religious practices, the one exception was a recent convert to a fundamentalist church now proselytizing in the area.

The Texan group is composed of migrants who came from Eastern Texas during the drought and depression of the early 1930's. With minor exceptions the households are farms on contiguous sections headed by persons who as young adults made the earlier move, or by their older children who have more recently married. Due to the short growing season and lack of rainfall, the cultivation of pinto beans has developed into the major cash crop. The ten couples who participated in this study were members of the ranking Presbyterian clique in the community.<sup>3</sup>

The ten couples selected for study in the Mormon village were chosen from the most active participants in the affairs of the local church. Religious teachings which exercise a pervasive effect upon local social organization specifically stress the role of the husband as the head of the family. The position of the church is stated in different ways in quotations similar to the following:

There must be a presiding authority in the family. The father is the head, or president, or spokesman of the family. This arrangement is of divine origin. It also conforms to the physical and physiological laws under which humanity lives. A home, as viewed by the Church, is composed of a family group, so organized as to be presided over by the father, under the authority and in the spirit of the priesthood conferred upon him.<sup>4</sup>

This patriarchal order has its divine spirit and purpose, and those who disregard it under

<sup>2</sup> Clyde Kluckhohn gives a brief description of the Navaho studies which are now considered a part of this project in the introduction to A. H. and D. C. Leighton, *Gregorio, the Hand Trembler*, Papers to the Peabody Museum, Cambridge, Mass., 1949.

<sup>3</sup> A forthcoming publication by Evon Z. Vogt will describe the social organization of this community in detail.

<sup>4</sup> John A. Widtsoe, *Priesthood and Church Government*, Salt Lake City, 1939, p. 81.

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one pretext or another are out of harmony with the spirit of God's laws as they are ordained for recognition in the home. It is not merely a question of who is perhaps best qualified. Neither is it wholly a question of who is living the most worthy life. It is a question largely of law and order, and its importance is seen often from the fact that authority remains and is respected long after a man is really unworthy to exercise it.<sup>5</sup>

Corresponding prescriptions for the wife's role emphasize that she should above all else be a mother, for "motherhood is the noblest, most soul-satisfying of all earthly experiences." Mormonism has a this-worldly orientation, divine grace is attained through effort, and the symbol of progress is the advancement the man makes in the priesthood and in extending his flocks and fields. The woman is not eligible for membership in the priesthood and her status is coupled with that of her husband both in her present life and in the next, by the regular Temple marriage. From the incomplete evidence now available, Mormon women of this community do not appear to have important land-holdings nor independent sources of income, and accounts of women's participation in church activities confirm the correspondence of women's current attitudes with the church writings quoted above. The historic emphasis by Brigham Young and others on woman's education and political participation was always hedged by the general reservation that motherhood should not be interfered with—the women of the community in question strongly emphasize this reservation.

In Navaho mythology and folklore the actions imputed to women contrast sharply with the emphasis of Mormon theology. For the Navaho the women become major charismatic figures.<sup>6</sup> Marriage customs are

also consistent with this conception of the Navaho woman as an active and demanding person. On the morning after a Navaho wedding the groom runs a foot race with his bride. The cultural interpretation is that "the one who wins will become rich."<sup>7</sup> This practice is quite different from the familiar custom in which the bride is passively carried over the threshold, and it is also a commentary on the independence of the economic fortunes of Navaho marriage-mates.

In summary, the favored position of the Navaho woman in contrast to the Mormon woman was judged in terms of economic, religious, and kinship considerations to be quite unequivocal. Between Texan and Mormon women there is less difference, but in terms of holding church office and the present possession of productive land and semi-professional jobs, the women in the Texan community appear to be more favored than the Mormon women. On the basis of this analysis it was predicted that Navaho women would win the highest percentage of the decisions and the Mormon women the smallest.

*Experimental Procedure.* The area under study had no electrification, and since it was impractical to attempt to bring the subjects to an observation room, the field sessions of the experimental procedure were recorded by portable sound equipment powered from a truck. Although the subjects were separated from the experimenter and other persons, they knew that their voices were being recorded. The task was explained to the Navahos by an interpreter. An appropriate picture was presented for each question and underneath the illustration there were pockets representing the three reference couples. The Navaho would place his marker in the pocket which represented the couple of his choice. In those instances in which there had been a difference between the choice of the man and wife, the illustration was presented again to the two of them with their markers in separate pockets.

<sup>5</sup> Joseph F. Smith, *Gospel Doctrine*, Salt Lake City, 1929, p. 359.

<sup>6</sup> These include Changing Woman, Spider Woman and Salt Woman. Blessing Way, the most frequently repeated ceremonial, stresses that each of the four poles of the hogan represent still different female divinities. Kluckhohn and Leighton comment that this practice "speaks volumes for the high place of women in the traditional conceptions." *The Navaho*, Cambridge: Harvard University Press, 1947, p. 56.

<sup>7</sup> Gladys A. Reichard, *Social Life of the Navaho Indians*, New York: Columbia University Press, 1928, p. 141.

They were requested to combine their markers in the position which best represented their joint opinion. Some questions were changed somewhat by translation into Navaho; for example, the question, "Which family is the most religious?" became "Which family follows the 'Navaho Way' best?" It was not felt that these changes would significantly modify the results here presented. These recordings were transcribed and, in the case of the Navaho, translated into English.

The written protocols were analyzed to determine the number of acts used by each participant and the distribution of these acts in terms of interaction process categories.<sup>8</sup> This information plus knowledge of the number of decisions won by each participant provides the basis for the analysis presented below.

TABLE 1. DECISIONS WON, BY SPOUSE AND CULTURE

Culture	Number of Couples	Decisions Won By:	
		Husband	Wife
Navaho	10	34	46
Texan	10	39	33
Mormon	10	42	29

*Findings.* We present in Table 1 the sum of the decisions won by the husbands and wives in each of the three cultures. The appropriate null hypothesis is compounded of two elements: (a) the proposition that the Mormon wives win an equal or greater number of decisions than their husbands ( $p=.007$ ); and (b) the proposition that Navaho husbands win an equal or greater number of decisions than their wives ( $p=.16$ ). Since the combined probability associated with these two propositions is less than .01, we reject the null hypothesis and conclude that we were able to predict the balance of decision-winning from our study of the comparative social and cultural

organization of the groups from which our sample was drawn.

Having to this limited degree established the validity of the technique, we are encouraged to inquire further into elements of behavior in the small group situation which are linked with decision-winning. Our earlier experience had indicated a very strong relationship between decision-winning, or leadership, and talking-time in *ad hoc* groups of four persons.<sup>9</sup> In the present instance two-person primary groups are involved. From a broader study of the rank characteristics of participants in groups ranging in size from two to ten persons it is known that differentiation in speaking-time in two-person groups is relatively less than it is in larger groups, hence it is probable that the relation between speaking-time and decision-winning is less clearly defined in two-person than in larger groups.<sup>10</sup> There was no compelling rationale for predicting the effects of the primary relationships upon "speaking and decision-winning." By combining the ten cases observed at Cambridge with the thirty cases from the field and eliminating the six cases in which the decisions were split evenly, we obtain the thirty-four cases compared in Table 2. The null hypothesis of independence between talking most and winning may be rejected at the .05 but not the .01 level.

TABLE 2. DECISIONS WON AND TALKING-TIME FOR 34 MARRIED COUPLES

Spouse Who Talked Most	Spouse Who Won Most	
	Husband	Wife
Husband	14	5
Wife	5	10

<sup>9</sup> Bass reports a correlation of .93 between the time a participant in an eight-man group spent talking and the votes he received from observers for having demonstrated leadership. See Bernard M. Bass, "An Analysis of Leaderless Group Discussion," *Journal of Applied Psychology*, 33 (1949), 527-533.

<sup>10</sup> See Robert F. Bales, Fred L. Strodbeck, Theodore M. Mills, and Mary E. Roseborough, "Channels of Communication in Small Group Interaction," *American Sociological Review*, 16 (August 1951), 461-468.

<sup>8</sup> For a description of the categories used see Robert F. Bales, "The Analysis of Small Group Interaction," *American Sociological Review*, 15 (April 1950), 257-264.

To approach a more systematic description of the interaction characteristics of the spouse who talks most, we have selected the 24 cases in which there was a significant difference between the number of acts originated by the husband and the wife. We find that the most talking spouse tended more frequently to *ask questions*, carry out *opinion and analysis*, and make *rewarding remarks*. As Simmel suggested, in a dyad there can be no coalitions—the speaker does not have alternative audiences, so the “threat of withdrawal” is generally a more compelling adjustmental device in two-person than in larger groups. While we do not as yet have norms by group size for category usage on a common task, the unexpected finding in the present study that the most active participant is significantly high in question-asking gives us further insight into how withdrawal is anticipated and prevented. The finding that the frequency of opinion and analysis acts is higher for the most talking person is in agreement with Bales’ notion that acts of this type have a central generative function which results in their being heavily represented in the profile of the most talking person in groups of any size.

The categories which discriminate the profile of the least talking participants are, in order of magnitude, the following: simple *acts of agreement*, *aggressive acts* designed to deflate the other actor’s status, and simple *disagreements*. Taken together, these characteristics suggest the passive agreeing person who from time to time becomes frustrated and aggresses.

Concerning cultural differences in category usage, the Navahos gave *opinion, evaluation*, and *analysis* acts during the solution of their differences only one-half as frequently as the Mormon and the Texan group. As a result they required on the average fewer acts per decision (8 in contrast with 30 for the other groups) and the reasoning and persuasion in their protocols seemed extremely sketchy. They did not emphasize

the arguments that might bear upon the issue, they tended to reiterate their choices and implore the other person to “go with them;” “go together,” or simply consent. This is in marked contrast with the other couples who appeared to feel that they had to give a reasoned argument to show that they were logically convinced, even when they were giving in to the other person. It is a matter for further research to determine if other “traditional” people show a similar tendency to minimize analysis in social problem solving.

For the Texans it was a rational exercise, sometimes directly commented upon, to see that the decisions came out even, the standard deviation between spouses in decisions won was only 1.3. The Mormons were less concerned with equality, the comparable figure is 2.1, and among the Navaho there were marked differences between spouses, the standard deviation being 5.1. An analysis of the way in which couples tended to go from orientation acts to evaluative acts before making suggestions for a final disposition of the difference, the so-called phases in interaction, will be presented in a later paper.

*Summary.* The essence of the revealed difference technique here described consists of: (a) requesting subjects who have shared experiences to make individual evaluations of them; and then, (b) requesting the subjects to reconcile any differences in interpretations which may have occurred. It has been shown that the disposition of these reconciled decisions is related both to power elements in the larger social and cultural organization and amount of participation in the small group situation. It is believed that other couples as well as parent-child, foreman-worker, and similar relationships may be profitably studied with the technique, since it appears not only to reveal the balance of power, but also to produce a sample of interaction in which modes and techniques of influence can be studied by methods of content and process analysis.



# A PRELIMINARY STUDY OF THE SIZE DETERMINANT IN SMALL GROUP INTERACTION\*

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**I**F a small human group is defined as one in which the members, integrated by direct communication demands, interact functionally and continuously toward the achievement of an end, then the structure resulting from such interaction is a unitary system of relationships in which the factor of size (number of participants) is one of the determinants of the system. In a word, the size determinant is an integral factor of small group interaction, the fundamental question being the relation of the magnitude of the variable to the range and complexity of the interactory field.

The scope of the present study is limited

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to an investigation of the size determinant in certain classes of small groups. Information was obtained from published sources and by correspondence for certain groupings in two of our basic institutions, namely, governmental and economic. Data for other groupings—play, work, conversational, and other—were secured through field observation.

Table 1 gives the ranges of group size and mean group sizes for the subcommittees of the United States Senate, and of the United States House of Representatives; committees, boards and so on of the State of Oregon, and of Eugene, Oregon governments; and subgroups in the officer and board of director organizations of four large corporations. The mean group sizes, it is noted, range from 4.7 to 7.8.

Subgroupings were chosen as a basis for determining group size because of the well-known fact that (mere membership in a group is an insufficient criterion of participation by the members in the group's

TABLE 1. SOME GROUPINGS IN THE UNITED STATES, STATE OF OREGON, AND EUGENE, OREGON GOVERNMENTS, AND IN FOUR LARGE CORPORATIONS

Organization	Number of Groups	Range of Group Size	Mean Group Size
U. S. Senate Subcommittees of 11 Committees. <sup>1</sup>	46	2-12	5.4
U. S. House of Representatives Subcommittees of 14 Committees. <sup>2</sup>	111	3-26	7.8
State of Oregon Executive, Legislative, Judiciary, Boards, Departments, Commissions. <sup>3</sup>	96	2-14	5.7
Eugene, Oregon Executive, Council Members, Committees, Boards. <sup>4</sup>	19	3-11	4.7
Subgroups in Officer and Board of Director Organizations of Four Large Corporations. <sup>5</sup>	29	3-9	5.3

<sup>1</sup> Source: Library of Congress Report, February 14, 1947.

<sup>2</sup> Source: House Reports for 81st Congress.

<sup>3</sup> Source: Oregon Blue Book, 1949-50.

<sup>4</sup> Source: City of Eugene Reports, 1949.

<sup>5</sup> Utilities, Insurance, Oil, Chemicals; data from secretaries of the corporations March, 1950.



function.) It was not assumed, however, that the subgroupings would not include some among their members whose participation was less than required of the members by the function in question. Rather the subgroup was selected as the unit because it was believed that non-participation by the members would be at a minimum in such groups, thus providing a firmer basis for determining the sizes of empirical groups. Support for this point of view came from the secretary of one of the world's largest banks who was insightful enough into our problems to differentiate action-taking from non-action-taking subgroups among the officers and board of directors of his bank. The data secured from this source were as follows:

	Number of Groups	Range of Group Size	Mean Group Size
Action-taking subgroups	12	4-10	6.5
Non-action-taking subgroups	9	10-23	14.0

The mean of 6.5 for the action-taking subgroups is clearly much smaller than the 14.0 mean for the non-action-taking subgroups. "We have found," wrote the secretary, "that committees should be small when you expect action and relatively large when you are heading for various points of view, reactions, etc." The mean of 6.5, further, compares with the means of 5.4, 7.8, 5.7, 4.7 and 5.3 found in Table 1. Some estimation is hereby given of the average size of the classes of action-taking empirical groups which were sampled.

There is evidence that certain functions may be more effectively performed by groups whose average size is less than the means given above. Support for this point is available from two sources. First, information supplied by the secretaries of the four large corporations included descriptive material of informal groupings among the leaders, officers and directors of their organizations. In general, the picture drawn by the secretaries was as follows: leaders with common problems met informally in groups of two, three

or more at any time the need arose. The issues were discussed, a consensus reached, and recommendations were presented to the formal authority having jurisdiction over the matter.

The second source was a series of field observations made of group sizes in Eugene and Portland, Oregon. This block of data is based on a much larger sample and is also less impressionistic and much more systematic than the information provided by the corporation secretaries.

Two sets of field observations were made: the first in February, 1950 in Eugene (population about 38,000); and the second in June-July, 1950 in Eugene and in Portland (population about 400,000). The observations in both sets took the form of merely counting group sizes—groups in which the members were in face-to-face interaction as evidenced by the criteria of gesticulation, laughter, smiles, talk, play or work. Individuals who merely occupied contiguous space were not counted as members of a group. Finally, all observations were carried out by teams of two, namely, a counter and a recorder. Observations were made, in most cases of Eugene and all of Portland, over one-hour periods. An hour of observing consisted of counting continuously for 10 minutes, pausing for 5 minutes, and counting again, repeating the 10-5 minute pattern over the 60 minute period. A few types of group activity were observed for only one-half hour, most types for 1, 2, or more hours, the greatest number being 7 hours for Portland pedestrians. In no case was the period of continuous observing longer than 2 consecutive hours.

The groups observed were classified for generalizing purposes as follows:

1. Informal—shopping, conversing, playing, walking.
2. Simulated Informal—acting in stage plays, movies, or performing in a radio broadcast.
3. Work—buying, construction, repair.

Table 2 shows the members of groups, and ranges and means of group size for these three classes of data. It is noticed at once

that the ranges are narrower and the means smaller than those given in Table 1.<sup>1</sup>

TABLE 2. RANGES AND MEANS OF GROUP SIZE, BY CLASSES OF GROUPS\*

Class of Groups	Number of Groups	Range of Group Size	Mean Group Size
Informal <sup>1</sup>	7,405	2-7	2.41
Simulated			
Informal <sup>2</sup>	176	2-5	2.37
Work groups <sup>3</sup>	1,548	2-6	2.35

\* Observations were made in Eugene and Portland, Oregon, in Winter and Spring, 1950.

<sup>1</sup> Observation of groupings among: pedestrians (a.m., p.m., evening); shopping (4 department stores, food market); free play (14 public schools, 3 nurseries, 4 summer playgrounds); public gatherings (carnival, 2 picnics, formal reception, swimming pool, 2 basketball game intermissions, 2 church socials, train depot).

<sup>2</sup> Observations of groupings among: two stage plays; four movies; and listening to the broadcast of a radio station from 6 a.m. to midnight.

<sup>3</sup> Observations of groupings among: buying (4 department stores); construction (campus building); repair (railroad roundhouse).

The frequency distributions of group sizes for the three classes of groups—Informal, Simulated Informal, and Work—are presented in Table 3. An examination of the distributions will disclose that all distributions exhibit the J-curve form.<sup>2</sup>

<sup>1</sup> These averages compare with those of the following reports: 3.04, 3.40 in Dorothy S. Thomas and Associates, *Some New Techniques for Studying Social Behavior* (New York: Teachers College, Columbia University, 1929), p. 80; 3.1, 3.3 in Ronald Lippitt, "An Experimental Study of the Effect of Democratic and Authoritarian Group Atmosphere," *Studies in Child Welfare*, Vol. IV, University of Iowa Studies, Iowa City, Iowa, 1937, p. 155; group size proportions in A. S. Salusky, "Collective Behavior of Children at a Preschool Age," from the Institute of Educational Research, Kharkov, Ukraine, reported in *Journal of Social Psychology*, Vol. I, 1930, p. 371; and 4-6 in Ferenc Merei, "Group Leadership and Institutionalization," *Human Relations*, Vol. II, 1949, p. 25. In the Salusky and Merei papers it may be inferred that the average size is about 3.

<sup>2</sup> These regularities conceal minor deviations from the J-curve which occurred in the tails of 3 of the 48 component distributions. There is reason to believe, however, that these deviations were due

TABLE 3. FREQUENCY DISTRIBUTION OF GROUP SIZES, BY CLASSES OF GROUPS

x	Informal <sup>1</sup>		Simulated Informal		Work	
	f	%N	f	%N	f	%N
2	5,263	71.07	125	71.02	1,104	71.32
3	1,496	20.20	38	21.59	357	23.06
4	471	6.36	12	6.82	73	4.72
5	133	1.80	1	.57	12	.78
6	41	.56			2	.12
7	1	.01				
	N=7,405		N=176		N=1,548	

<sup>1</sup> The frequency of 1 for group size 7, it is noted, represents the only instance when a group size larger than 6 was observed. The case in question took the form of 2 adults and 5 children. More careful observation might have discerned that this group was actually composed of two or more subgroups as defined by our criteria of the small group.

A brief word may be said about the possible theoretical meaning of our findings, especially as suggested by the results of the field observations. The field data, it is recalled, were taken from a variety of situations obtaining in two urban areas, one small and the other large. Observations were carried out in the winter and in the spring. Further, an attempt was made to include age representation from childhood (age 2) to those who were not too senile to participate in the kinds of group activities observed. No differentiation was made as to sex. Finally, the distributions of group size which derived from this variety of observations were more uniform than their diverse sources might perhaps lead one to infer on *a priori* grounds.

This uniformity suggests that certain conditions, not primarily dependent on age, space, motivation, or social situation, operate

to observer error or to an insufficient number of observations.

The J-Curve uniformities displayed by the distributions prompted us to search for a statistical model which might be appropriate for representing distributions of small group size. The Poisson model was applied to our empirical distributions and was found to fit in 70 per cent of the distributions when a  $X^2$  test at the 1 per cent of significance was used.

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to produce the general patterning of group size which was observed. We speculate that the controlling factors are the motoric, perceptive and thinking variables. If these variables are conceived as the components of organized (directed) action per unit of time, then it follows that systems of interaction (groups) are limited by the number of relationships individuals are able to maintain continuously within a system. That the number of such relationships is not great is evidenced by the fact that the group sizes observed were consistently small.

Kephart has shown that if all that takes place in the interaction between two individuals, or between an individual and a subgroup, or between two subgroups is described as a *single* relationship (clearly an oversimplification, but a useful one for certain purposes), then "the possible number of relationships that can exist in a given group at a given time" is<sup>3</sup>

$$P.R. = \frac{3^N - 2^{N+1} + 1}{2}$$

where N is the number of members, and P.R. is the number of potential relationships between the members. Table 4 illustrates the rapid increase in P.R. which occurs with consecutive increases in group size, and at the same time compares these increases with the decreases in the percentages of group size which occur with consecutive increases in group size. (The percentages are derived from a summation of the three empirical distributions given in Table 3.)

Our empirical observations indicate that groups characterized by face-to-face and spontaneous interaction tend to gravitate to

TABLE 4

Group Size	Potential Relationships (by Kephart's Formula)	Percentage of Cases (from Empirical Observations)
2	1	71.11
3	6	20.71
4	25	6.09
5	90	1.60
6	301	.41
7	966	.01

the smallest size, i.e., 2—to the point of the least number, i.e., 1, of relationships (as defined by Kephart) required for interaction. It is possible that groups above size 2 manifest increasing instability per unit increase in size. In the course of the field work the author noticed that groups of 5 and above were very unstable and rather quickly divided into subgroups.<sup>4</sup>

By way of a final word it may be said that the evidence produced by this study, and supported by the reports of other investigators, leads to the conclusion that freely forming and unforming groups undergoing continuous interaction are very small, falling within a size range of about 2-7, and having an average size of about 3.<sup>5</sup>

<sup>4</sup> This fact, also reported by others, suggests that the stability (life) of unitary systems characterized by spontaneity may be expressed by the general relation

$$\text{Stability} = \frac{\text{Time}}{\text{Group Size}}$$

This formulation, of course, requires testing.

<sup>5</sup> These findings suggest that larger "groups" of whatever complexity are actually molecular arrangements of small groups (unitary systems), and could be analyzed into their unitary system components by postulating certain structural modes as entering into the composition of inter-group organization. See the author's "Some Elements in a Theory of Small Groups," *Research Studies*, State College of Washington, Vol. XVIII, 1950, pp. 150-51.

<sup>3</sup> William M. Kephart, "A Quantitative Analysis of Intragroup Relationships," *American Journal of Sociology*, 55 (May, 1950), 548.

## SOCIAL CLASS AND SOCIAL MOBILITY FACTORS RELATED TO MARITAL ADJUSTMENT

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IN any stratified social order, marriage usually implies equality of status. Although the American class structure considered in this paper is not so rigid as the caste system discussed by Kingsley Davis in "Intermarriage in Caste Societies,"<sup>1</sup> the fact remains that strata exist and the family is placed in the class structure as a unit. This means that if a man and woman of different social levels marry, there must generally be a shift of status for one or both of them.

The question arises: Does this necessary shift of status affect the subsequent adjustment of the spouses? Ruesch, Jacobson, and Loeb point out the feelings of stress, frustration, and confusion which often accompany the acculturation of immigrants.<sup>2</sup> The greater the degree of culture difference, the greater the maladjustment of the individual is likely to be. The differences in the characteristics of the social classes in our society indicate that these classes are different cultural groups. A person moving from one class to another must go through a process of acculturation similar to, though perhaps not so extreme as, that of an immigrant.

In some cases a shift of status of one or both spouses has taken place before marriage. A certain amount of stress and insecurity in social situations may thus be brought into the marriage relationship by one or both partners. This possibility suggests a second major problem: Do the premarital mobility patterns of the spouses affect the subsequent marital adjustment?

A fairly large group of cases which had some rating of marital adjustment and some

good indication of the subject's social class was needed in order to examine these problems. The schedules Burgess and Cottrell used in their study of marital prediction provided such a group of cases.<sup>3</sup> Each of their 526 cases had a marital adjustment score which they had worked out with the help of statistical techniques. Most of the cases had data which made possible an estimate of the social class level of the subjects.

No detailed account of the collection of the data and the characteristics of the group will be given here, since Burgess and Cottrell have already presented this information in Chapter II of their book.<sup>4</sup> It is important to note that the great majority of the subjects were in their twenties or early thirties and that the time since marriage was fairly short (less than seven years) in all cases.

### SOCIAL CLASS RATINGS AND ADJUSTMENT INDEX

The McGuire-Loeb modification of W. L. Warner's Index of Status Characteristics (I. S. C.) was used to determine the class status of each subject.<sup>5</sup> The schedules of 523 couples were consulted. Only those cases were used where a single, definite class placement could be made with reasonable

<sup>3</sup> Ernest W. Burgess and Leonard S. Cottrell, *Predicting Success or Failure in Marriage*, New York: Prentice-Hall, Inc., 1939.

<sup>4</sup> *Ibid.*

<sup>5</sup> The calculation of Warner's I. S. C. and the rationale on which it is based are presented in Part III of W. Lloyd Warner, Marchia Meeker, and Kenneth Eells, *Social Class in America*, Chicago: Science Research Associates, Inc., 1949. The McGuire-Loeb version may be found in Carson McGuire, "Social Status, Peer Status, and Social Mobility," Memorandum for the Committee on Human Development and supplement to *Social Class in America*, 1949, pp. 7, 8.

<sup>1</sup> *American Anthropologist*, 43 (1941), 376-395.

<sup>2</sup> Jurgen Ruesch, Annemarie Jacobson, and Martin B. Loeb, *Acculturation and Illness*, Psychological Monographs, Vol. LXII, No. 2, 1948.



confidence.<sup>6</sup> This provided a maximum total of 428 husbands and 417 wives. Some of the tables included here have a smaller number of subjects, since they pertain to questions or classifications which did not apply to all the subjects.

Four social class levels—upper-middle (UM), lower-middle (LM), upper-lower (UL), and lower-lower (LL)—had a sufficient number of cases to be useful in most of the tabulations.

The concept and measurement of marital adjustment used by Burgess and Cottrell, as well as the adjustment scores which they derived for each of their cases, were assumed without modification. The scores were likewise classified into the same GOOD (160–199), FAIR (120–159), and POOR (20–119) adjustment categories which Burgess and Cottrell used in almost all of their tables in *Predicting Success or Failure in Marriage*.<sup>7</sup>

#### SOCIAL CLASS AND MARITAL ADJUSTMENT

Table 1 shows an evident trend in the case of both the husbands and wives for the

<sup>6</sup> This rigorous approach reduced the size of the sample; however, it appeared that the results of the analysis could be stated with more confidence if the composition of the groups being compared was fairly uniform.

<sup>7</sup> Burgess and Cottrell, *op. cit.*

marital adjustment score to increase as we move up the social class scale. Testing this trend by the Chi-square method in comparison with a hypothesis of no relationship between social class and adjustment shows a significance at the 1 per cent level.

This finding is essentially in agreement with some of those of Burgess and Cottrell. Although they did not use any overall measure of social class for comparison with adjustment, Burgess and Cottrell did examine many of the individual factors which are known to be related to class level. Education,<sup>8</sup> amount of organizational membership,<sup>9</sup> character of neighborhood,<sup>10</sup> and degree of economic security,<sup>11</sup> are all positively correlated with class level. That is, in each case the part of the scale related to higher class status (e.g. more advanced education) included more couples who are well-adjusted than appear in the lower end of the scale (e.g. little education). The relationship of occupation to adjustment is less clear, because Burgess and Cottrell used an occupational classification which is only loosely related to the prestige value of the occupations. Executives and managers of large busi-

<sup>8</sup> *Ibid.*, pp. 121, 391.

<sup>9</sup> *Ibid.*, p. 126.

<sup>10</sup> *Ibid.*, p. 132.

<sup>11</sup> *Ibid.*, p. 261

TABLE 1. DISTRIBUTION OF THE HUSBANDS AND WIVES ACCORDING TO SOCIAL CLASS AND ADJUSTMENT INDEX

Social Class at Marriage		Adjustment Scores of Couples							
		Number				Percentage			
		Good	Fair	Poor	Total	Good	Fair	Poor	Total
Husbands	UM	98	58	32	188	52.1	30.9	17.0	100.0
	LM	62	44	49	155	40.0	28.4	31.6	100.0
	UL <sup>a</sup>	27	17	28	72	37.5	23.6	38.9	100.0
	LL <sup>a</sup>	3	5	5	13	23.0	38.5	38.5	100.0
	Total	190	124	114	428 <sup>c</sup>	....	....	....	....
Wives	UM	63	32	25	120	52.5	26.7	20.8	100.0
	LM	106	71	53	230	46.1	30.9	23.0	100.0
	UL <sup>b</sup>	16	15	28	59	27.1	25.4	47.5	100.0
	LL <sup>b</sup>	1	2	5	8	12.5	25.0	62.5	100.0
	Total	186	120	111	417 <sup>c</sup>	....	....	....	....

<sup>a</sup> The cells in these two rows were combined in the Chi-square analysis.

<sup>b</sup> The cells in these two rows were combined in the Chi-square analysis.

<sup>c</sup> In this and in subsequent tables the total number of cases is less than 523 because the "unknown" and "indeterminate" cases are omitted.

nesses and professional people, especially school teachers, show slightly better scores than clerical, sales, small business, and skilled trades people.<sup>12</sup> Terman also found a slight positive relationship of marital happiness to education,<sup>13</sup> but no certain relationship to occupation<sup>14</sup> (again occupation was not classified according to prestige level).

When the social class background of each individual subject (that is, the social class of the spouse's parents) is tabulated, no class level trend in relation to adjustment is apparent (Table 2). The only fairly sharp difference lies between the lower-lower class and the remainder of the group. Lower-lower status has a slight negative relationship with marital adjustment. It thus appears that the social class of the spouses' parents *per se* has little relationship to the adjustment of the spouses.

#### SOCIAL CLASS DIFFERENCE AND MARITAL ADJUSTMENT

In over half the cases in this study in which the difference or similarity of the social class of the spouses was established, the spouses were of the same social class at the time of marriage. (See Table 3.) Burgess and Cottrell state<sup>15</sup> that marriage tends to

take place within a given cultural group and this has probably been the common finding in studies of marriage.<sup>16</sup>

Although most of the marriages take place within a given class, a substantial number of cross-class marriages are represented in Table 3. How do they compare in adjustment to same-class marriages? According to the hypothesis previously discussed, the cross-class marriages are likely to cause greater stress to the persons involved. How does this reflect on their relative adjustment? The percentage distribution in Table 3 shows that the adjustment scores tend to be higher in the case of same-class marriages. Testing this relationship by the Chi-square method<sup>17</sup> shows a significance at the 1 per cent level. This result suggests that the stress of a rapid shift in class values required by a cross-class marriage has a negative influence on the adjustment of that marriage.

In a cross-class marriage either the hus-

<sup>16</sup> Robert T. McMillan, "Farm Ownership Status of Parents as a Determinant of Socio-economic Status of Farmers," *Rural Sociology*, 9 (June, 1944), 151-160. McMillan reports a strong tendency of farmers to marry within their own status group (defined by farm ownership) even where a marked disparity of the sex ratio made such pairing difficult.

<sup>17</sup> The corresponding cells for "1 class apart" and "more than 1 class apart" had to be combined because of the very small number of cases in the "more than 1 class apart" category.

<sup>12</sup> *Ibid.*, p. 136.

<sup>13</sup> Lewis M. Terman, *Psychological Factors in Marital Happiness*, New York: McGraw-Hill Book Co., 1938.

<sup>14</sup> *Ibid.*, p. 169.

<sup>15</sup> Burgess and Cottrell, *op. cit.*, p. 77.

TABLE 2. RELATIONSHIP OF THE SOCIAL CLASS OF THE PARENTAL FAMILY TO THE ADJUSTMENT INDEX OF EACH SUBJECT

Social Class at Marriage	Adjustment Score of Each Subject							
	Number				Percentage			
	Good	Fair	Poor	Total	Good	Fair	Poor	Total
Subject's parents								
UM	67	43	40	150	44.7	28.6	26.7	100.0
LM	106	70	74	250	42.4	28.0	29.6	100.0
UL	87	54	45	186	46.8	29.0	24.2	100.0
LL	17	5	25	47	36.2	10.6	53.2	100.0
Total	277	172	184	633*	....	....	....	....

\* Of the 1,046 subjects (523 husbands and 523 wives) only 633 gave sufficient information about their parents to permit class ratings to be made. Each parental family was status-identified by the man's occupation, source of income, and education.

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TABLE 3. DISTRIBUTION OF TOTAL CASES ACCORDING TO ADJUSTMENT INDEX AND SIMILARITY OR DIFFERENCE OF THE SOCIAL CLASS OF THE SPOUSES

Social Class at Marriage	Adjustment Score of Each Subject							
	Number				Percentage			
	Good	Fair	Poor	Total	Good	Fair	Poor	Total
Spouses of same class at time of marriage	115	56	44	215	53.5	26.0	20.5	100.0
Spouses 1 class apart at time of marriage	56	50	54	160	35.0	31.2	33.8	100.0
Spouses more than 1 class apart at time of marriage	3	8	10	21	14.3	38.1	47.6	100.0
Total	174	114	108	396	....	....	....	....

band or the wife may be of the higher social class at the time of marriage. Does the effect on marital adjustment differ with the sex of the spouse of superior status? In Table 4 the direct comparison is made of all the cases in which the husband was of the higher class at marriage, with all those in which the wife was of the higher class at marriage. The wife-high cases seem to be more unfavorable to marital adjustment than the husband-high cases, although both show a tendency to lower scores than same-class marriages. Using the Chi-square technique we may test the hypothesis: Cross-class marriages in which the husband is of the higher social class generally show better adjustment than those in which the wife is

of the higher class. The level of significance proves to be very low ( $0.30 < P < 0.50$ ). The relationship is obscured by the fact that the class differences represented can have different origins. For example, a woman may have been mobile before marriage past the level of her future husband or she may have acquired her higher status from her parents. This problem will be further discussed in the section on mobility patterns.

Despite the low statistical significance of the relationship between wife-high and husband-high cross-class marriage, the relationship is in keeping with findings on this point in other studies. McMillan finds that in the case of marriages across class lines, it was most often the wife who "married up" the

TABLE 4. COMPARISON OF THE HUSBAND-HIGH CROSS-CLASS MARRIAGES WITH THOSE IN WHICH THE WIFE IS OF THE HIGHER CLASS

Social Class at Marriage	Adjustment Score of Each Subject							
	Number				Percentage			
	Good	Fair	Poor	Total	Good	Fair	Poor	Total
Husband 1 or more classes higher than the wife at marriage	41	38	37	116	35.3	32.8	31.9	100.0
Wife 1 or more classes higher than the husband at marriage	18	20	27	65	27.7	30.8	41.5	100.0
Total	59	58	64	181	....	....	....	....

status ladder.<sup>18</sup> To put it another way, the men seemed to be more willing than the women to accept a lower status spouse. Terman found that in his subjects the wives who were markedly superior to their husbands in education<sup>19</sup> had low happiness scores, while the scores were much higher in cases where the husbands were markedly superior in education.<sup>20</sup> James West points out that in all of the cross-class dating in "Plainville" the boy is of the higher class. A boy dating a girl of a lower class is frowned upon, but "for an upper-class girl to have a date with a lower-class boy would be inconceivable."<sup>21</sup>

Why is adjustment smoother when the man enters marriage at the higher status than when the woman does so? An important finding of Burgess and Cottrell gives a clue for further study on this point. The major adjustment, in fact almost the entire adjustment in most marriages, is made by the wife.<sup>22</sup> Since an upward shift in class status carries some rewards and also entails fewer punishments than a downward shift, we would expect less stress in those cases where the wife had to move upward (that is, the

husband-high marriages) than in those where she was expected to shift her values downward (that is, the wife-high marriages).

In the case of differences in class background the results were unexpected. Table 5 shows no relationship of the adjustment score of the spouses to the social distance of their respective parents. That is, the adjustment of the spouses does not seem to be affected by the fact that their parental background is of the same or different class level.

The results presented in Table 5 indicate that the difference in the parental social status *per se* does not affect the marital adjustment of the spouses. This is a direct contradiction of the finding of Burgess and Cottrell that the closer the similarity of the family background, the better the marital adjustment of the spouses.<sup>23</sup> Why the difference? It is important to note that Burgess and Cottrell used a method of estimating parental status level which is different from the one used in this study. The latter uses Warner's social class concept and relies largely on the Index of Status Characteristics with the items: occupation, source of income, and education. Burgess and Cottrell used a numerical index of similarity in family backgrounds based on the weighted items: parents' religious preference, their church participation, their education, the father's occupation, the respondent's rating of their economic status, and the respondent's rating

<sup>18</sup> McMillan, *op. cit.*

<sup>19</sup> Education may serve as a crude index of class level.

<sup>20</sup> Terman, *op. cit.*, p. 191.

<sup>21</sup> James West, *Plainville, U. S. A.*, New York: Columbia University Press, 1945. In this rural village dating is generally looked upon as a forerunner to marriage.

<sup>22</sup> Burgess and Cottrell, *op. cit.*, p. 341.

<sup>23</sup> Burgess and Cottrell, *op. cit.*, pp. 82-85.

TABLE 5. DISTRIBUTION OF ADJUSTMENT SCORES OF COUPLES IN TERMS OF THE SOCIAL CLASS DIFFERENCE OF THEIR PARENTS

Social Class at Marriage	Adjustment Score of Each Subject							
	Number				Percentage			
	Good	Fair	Poor	Total	Good	Fair	Poor	Total
Couples' parents of same class	44	24	32	100	44.0	24.0	32.0	100.0
Couples' parents 1 class apart	48	30	30	108	44.4	27.8	27.8	100.0
Couples' parents more than 1 class apart	25	16	20	61	41.0	26.2	32.8	100.0
Total	117	70	82	269	....	....	....	....



of their social status. The last two items are subjective ratings which appeared rather unreliable when compared to the objective data provided by the schedules. Some attempt was made to examine the biases in the methods of determining status level to account for the apparently contradictory results. This examination was inconclusive. The difference in the results of the two studies probably lies in a difference of classification of occupation, education, and religion and a different weighting of these factors.

#### SOCIAL MOBILITY AND MARITAL ADJUSTMENT

Social mobility is defined in this study as the distance which a subject moved from his parents in terms of social class level. Thus, if the subject were of the same social class as his parents at the time of his marriage, he is classed as "non-mobile," if he is of a higher social class he is "upward mobile," and if he is of a lower social class he is "downward mobile." In over half the cases in which the mobility pattern could be identified one or both of the spouses were upward mobile. This high proportion probably results from the fact that Burgess and Cottrell included in their study a large number of subjects with advanced education.

The high proportion of cases including downward mobile subjects is harder to explain. Although no accurate estimates of downward mobility have been made, the 20 per cent of cases involving downward mobility in this study seems exceptionally high. It is likely that some of these are cases of "age-graded" mobility.<sup>24</sup> An example of age-graded mobility is the situation in which a man has established himself in a high status level, but his son is forced to start his business or professional career at a lower occupational level. The schedule data were not adequate for distinguishing between age-graded and permanently downgraded persons, so they had to be lumped together in the "downward mobile" category. This fact should be kept in mind wherever the down-

ward mobile group is used in later analysis.

Earlier in this paper the problem was posed: How does mobility prior to marriage affect later adjustment? Does the stress of this earlier culture shift make marital adjustment more difficult? In order to examine this problem the major mobility groups—non-mobile, upward mobile, and downward mobile—were compared to the total group.

The non-mobile group has almost the same proportional distribution as the total group. The upward mobile group actually shows better adjustment than the total group, although this difference proves to be non-significant when the Chi-square test is applied. The stress of the earlier upward culture shift does not appear to affect marital adjustment adversely. It must be remembered that at the time of marriage an upward movement in status has already been achieved. The upward mobile person has largely or entirely assimilated the values of his new position. If he (or she) marries a person of this new social level, he is likely to make his relatively new position more secure and thus improve his general social adjustment. If he has moved up quickly through the educational and occupational ladder, but has not yet assimilated the values of his new position, his marriage to a person at this level may facilitate the learning of new behavior. In their discussion of the acculturation of immigrants, Ruesch, Jacobson, and Loeb point out that the migrant who marries a native partner acculturates much faster than his fellow migrants. "Constant exposure to a model, and reward in terms of affection, apparently accelerates the acculturation process."<sup>25</sup>

The downward mobile group, on the other hand, shows a distribution of significantly lower scores ( $P < 0.02$ ) than the total group, despite the probable dilution of "age-graded mobility" cases.<sup>26</sup>

<sup>25</sup> Ruesch, Jacobson, and Loeb, *op. cit.*, p. 23.

<sup>26</sup> The strong social disapproval of downward mobility may make the intimate relationship of marriage less stable. More likely, perhaps, the downward mobile person is apt to be a rebel against convention, and to be a person who rejects respon-

<sup>24</sup> McGuire, *op. cit.*, pp. 3, 19.

The tables in the previous section indicated that spouses of the same social class level at the time of marriage scored higher in adjustment than those who were at different levels at marriage. Does this hold true regardless of the spouses' mobility patterns or do the mobility patterns contribute unequally to this relationship? To examine this question the cases within each of the major mobility groups were divided into those who were at the same level at marriage and those who were at different levels.

The "non-mobile" and "one spouse upward mobile" categories show a marked tendency for same-class marriages to have good adjustment compared with cross-class marriages. The Chi-square test shows that this relationship is significant at the 1 per cent level. The "both spouses upward mobile" group shows the same relationship, but the number of cases is too small for statistical analysis. The "downward mobile" group, on the other hand, shows no marked difference between the same-class and cross-class marriages. The slight trend toward better adjustment favoring the same-class marriages proves to be of low significance ( $0.30 < P < 0.50$ ). It appears that the unfavorable influence of downward mobility upon marital adjustment is so strong that the relationship of the class levels of the spouses is relatively unimportant.

Breaking the total group into a large number of mobility patterns left so few cases in each category that a further breakdown into "husband high" and "wife high" or "husband mobile" and "wife mobile" groups would make statistical analysis impossible. Nevertheless, some of the figures suggest explanations of relationships discussed earlier.

In the previous section it was noted that in cross-class marriages adjustment generally appeared poorer when the wife was of the higher class, than when the husband was of

sibilities. His downward mobility would then be a symptom of difficulties in maintaining stable emotional relationships with others. In this case, the unsatisfactory marital relationship would be only one reflection of a general personality pattern.

the higher class. The different mobility types are found to contribute very unequally to this relationship.

In the category "one spouse upward mobile, passing level of the other" the distribution is as follows:

	Good	Fair	Poor
Husband mobile	12	7	7
Wife mobile	2	2	8

Although the numbers are small the difference in distribution of "husband mobile" and "wife mobile" cases is extreme. When the wife is upward mobile and of higher class at marriage, the adjustment is markedly poor. That this poor adjustment is due primarily to the class difference rather than the mobility is shown by comparison with the tabulation of the category "one spouse upward mobile, reaching the level of the other":

	Good	Fair	Poor
Husband mobile	25	8	4
Wife mobile	15	5	0

In this tabulation the cases where the wife alone was upward mobile show no skewing toward poor adjustment; in fact, not a single one of the twenty cases falls in the "poor" classification. Also, in those cross-class marriages where there was no upward mobility on the part of either spouse before marriage, the distribution of adjustment scores is about the same for the husband-high and wife-high cases.<sup>27</sup>

<sup>27</sup> This might suggest that women who were upward mobile before marriage find the adjustment to a lower status husband more difficult than those who were not upward mobile. We may speculate that a young woman who moves upward in class status before marriage by means of the educational and occupational ladder is likely to desire a role other than (or in addition to) that of housewife and mother. If she marries a man of lower status, her position will appear to be one of superiority over her husband. Since most men in our culture find such a position ego-shattering and since a downward shift in status on the part of the originally upward-mobile wife is similarly difficult, such a marriage is likely to prove unsatisfactory.

SPECULATIONS AND SUGGESTIONS FOR  
FUTURE RESEARCH

In this paper marital adjustment has been examined in terms of certain relationships with the social class levels and mobility status of the spouses at marriage. Of these factors the one that appears to be of primary importance is the similarity or dissimilarity of the social class level of the spouses at marriage. In other words the chief question is: Are the spouses of the same social class or of different class levels at marriage? The former case is favorable to good adjustment, the latter unfavorable, in the population studied here. Whether the spouses are non-mobile or one or both are upward mobile, whether they are of the higher or lower classes, whether their parents are of the same or different social class levels, the most important factor is still the similarity of the class level of husband and wife at the time of marriage.

This does not mean that the other factors can be ignored. The social class level of the husband and wife in itself is related to adjustment. The subjects show progressively higher adjustment scores as we go up the social class scale. The values of the various social classes concerning marriage and family life may differ in such a way as to make for better adjustment between the spouses at the higher levels. Perhaps Burgess's concept of "companionship"<sup>28</sup> in marriage is found more often at the higher class levels. Downward mobility has a strong unfavorable influence on marital adjustment which tends to obscure other factors. Since in these cases the person was already downward mobile at the time of marriage, it is possible that the person is rebellious and rejects responsibility. His marriage relationships will therefore be unstable. Since the downward mobile group in this study is almost certainly diluted with cases of age-graded mobility, the unfavorable effect of downward mobility is probably even greater than appears in the analysis presented in this paper.

Surprisingly, difference in the social class background of the spouses (that is, the social class of their respective parents), does not appear to affect their adjustment. Neither does the husband's or wife's social class background *per se*, except for a possible unfavorable effect in the case of lower-lower class parents. Of course, the class level of the parents has an indirect influence insofar as it affects the class levels of the spouses. But the actual levels of the spouses at marriage, whether they have been inherited directly from the parents or have been moved into through some mobility route, seem to determine the success of the marriage.

Our data show this pattern, but do not explain it. Why should it exist? In attempting to account for it, it seems reasonable to assume that it is the present, operating values of the husband and the wife which determine how they behave and how they evaluate each other's behavior. Thus, it would not be the sociological fact of their social status which makes them happy or unhappy. Rather, it would be the class-typical day-to-day behavior which would tend to harmonize in the case of same-class marriages, and conflict in cross-class marriages.

These data further suggest that there is no necessary, mechanical inheritance of values from the parental family. Instead, they suggest that it is possible to learn a whole way of life which is different from that of the family one is born into; and to do it successfully, by the standards of the new social group. Thus it is not some mysterious, automatic reproduction of a sociological pattern; not some inexplicable, but inescapable, "background factor" which determines adult behavior and adjustment. Rather, the evidence indicates, as does so much other evidence, that the socially significant aspects of human behavior are largely learned. While the impress of childhood training is a powerful influence (and many of our mobile subjects may have been trained by a father or mother who envisioned and encouraged upward mobility), it remains that much of the acquisition of values and behaviors could and probably did occur outside the parental

<sup>28</sup> Ernest W. Burgess and Harvey J. Locke, *The Family*, New York: American Book Co., 1945.



home. In the case of our well-adjusted subjects with disparate backgrounds but similar class-status at the time of marriage, some such process seems to be the most likely explanation.

Thus, a fatalistic prediction that the children of Park Avenue and Railroad Street could not marry happily appears untrue. Some of them can and do learn to be a different kind of person than their parents were, and live the new role successfully with a marriage partner of the new class.

On the other hand, it may be that the ability or willingness to learn a new pattern of life declines after marriage. Those subjects who had been mobile, but married a person of a different social class (as of the time of marriage), did not show the same success in adapting to their marriage that they displayed in adapting to their new social position.

It may be that one's pattern of behavior is largely set by the time one marries. *On the average* (for this is a statistical deduction that should not be applied uncritically to a specific case), it may be that it is not nearly so easy to learn new behavior patterns after marriage as it was in the earlier years. This might be a function of increasing psychological rigidity with age.

There is another explanation, however, which may fit the facts better. We know that there are cases in which husband and wife jointly move up the social ladder *after* they are married. The ability to achieve the complex learning this requires does not disappear at the point of the nuptial ceremony. Acculturation to a new group can still occur. In a sense, the adjustment to a marriage partner might also be considered an acculturation process, insofar as it involves the modification of behaviors and attitudes. Yet, our data point out that the same people who successfully acculturate to a new social class find it harder to adapt to a spouse of a different social status. Cross-class marriages, even among the mobile, are a poorer risk than same-class marriages. It may be that the motivation to adapt to a higher social class is actually stronger, and that the rewards appear more desirable to

the individual, than is the case in adapting to the way of life of one's spouse. This is an unromantic explanation, but it seems very possible. Put bluntly, people may be more willing to change themselves in order to be successfully mobile than to make the changes necessary for a satisfying, stable marriage.

If nothing more, these data suggest certain deep differences among the different social classes; deep enough to make it relatively hard for two people of different classes to live together happily as man and wife. To illustrate, a person who believes in accumulating property and providing economically for the future (middle-class, especially upper-middle) would scarcely be able to agree in money matters with a person who prefers to spend all his money for immediate satisfactions (lower class, especially lower-lower). A person who seeks rather compulsively to impose "proper" behavior and attitudes on his children (middle class) would have difficulty in agreeing on child rearing problems with a person who had a more indulgent approach (lower class).<sup>29</sup>

As usual, a relatively small study such as this, using only statistical comparisons of groups, raises more questions than it answers. One hopes that they are different questions, based on a larger and clearer fund of knowledge than one had at the outset. The next step would preferably be an intensive study of married individuals, gathering the fullest possible information about childhood experiences and training, and the later influences and events that ultimately produce the adult behavior-value pattern present at the time of marriage. Further, we would need to know the crucial behaviors and attitudes that each partner shows within the successful and unsuccessful marriage.

A study, perhaps in collaboration with a family guidance service, could be set up to ask these questions. Interviews could be recorded for a selected sample of married couples. One or two projective tests, prefer-

<sup>29</sup> Of course, middle-class parents often hold a conscious philosophy of permissiveness, but the weight they give to "proper training," in practice, still contrasts markedly with lower-class parents.

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ably including the TAT, could be economically administered. Data on social background could be obtained in an interview, perhaps with a questionnaire as a guide.

Such material would allow us to examine in searching detail the motives and behavior

of the subjects; how they feel about their marriage, and why they feel that way; what culturally determined value-patterns distinguish the several social-class groups. The techniques are available, and the problem is a challenging one.

## THE FACTOR OF RELIGION IN THE SELECTION OF MARRIAGE MATES

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THIS paper is concerned with the question of intermarriage between religious groups. More specifically, it will investigate the rate of intermarriage between Catholics and non-Catholics. Religion has long been recognized as a barrier to intermarriage, although the traditional assumption has been that it was only one of the factors explaining the selection of marriage mates. Recently, however, the theory has been advanced that religious differences function as the chief basis of assortive mating.<sup>1</sup> Since intermarriage is the surest means of assimilation and the most obvious indication of its occurrence, it is maintained that the classic conception of a single "melting-pot" must be abandoned and a new hypothesis of assimilation substituted for it. This is the "triple-melting-pot" theory according to which assimilation will take place within three separate "melting-pots" based on the cleavage which exists between the three major religious groups in the country, namely, Protestants, Catholics, and Jews.

Kennedy and Hollingshead arrived at this new formula of assimilation after a thorough and enlightening study of the intermarriage pattern of the citizens of New Haven, Con-

necticut. Kennedy summarizes her findings as follows:

We shall, in other words, be able to state that, *while strict endogamy is loosening, religious endogamy is persisting and the future cleavages will be along religious lines rather than along nationality lines as in the past.* If this is the case, then the traditional "single-melting-pot" idea must be abandoned, and a new conception, which we term the "triple-melting-pot" theory of American assimilation, will take its place as the true expression of what is happening in the various nationality groups in the United States. This is the hypothesis which we believe the present paper proves true.<sup>2</sup>

Hollingshead agrees with this position:

... in most cases, marriages across religious lines involve the mixing of ethnic stocks. This is true whether Catholics and Protestants marry, or Jews and Gentiles, because the members of each religious group came from such different parts of Europe. From the viewpoint of assimilation, marriages across religious lines are crucial if the triple melting-pot is to become a single melting-pot. But as Kennedy's and our data show, we are going to have three pots boiling merrily side by side with little fusion between them for an indefinite period.<sup>3</sup>

These writers have advanced a very interesting hypothesis. If it were verified for the country as a whole, it would have considerable value for predicting the rate of assimilation of our various national minorities

<sup>1</sup> August B. Hollingshead, "Cultural Factors in the Selection of Marriage Mates," *American Sociological Review*, 15, (October, 1950), 619-627; Ruby Jo Reeves Kennedy, "Single or Triple Melting-Pot? Intermarriage Trends in New Haven, 1870-1940," *American Journal of Sociology*, 39, (January, 1944) 331-339.

<sup>2</sup> Kennedy, *op. cit.*, p. 332. Italics in the original.

<sup>3</sup> Hollingshead, *op. cit.*, p. 624.

since the religious affiliation of these groups is readily ascertainable. It shall be the purpose of this paper to test the validity of this "triple-melting-pot" hypothesis in regard to one of the major religious groups, the Roman Catholic. An adequate treatment of this highly important problem demands an answer to the following questions:

1. What is the mixed marriage rate for Catholics in the United States? For Catholics in Connecticut? Is there any evidence that the rate is increasing?
2. What are the principal factors determining the rate of mixed marriage in the various sections of the country?
3. Will Catholic marriage in the future tend toward the triple- or the single-melting-pot variety?

Before we take up these questions, a few remarks on the nature of the mixed marriage data are in order. There are fairly adequate data on all mixed marriages involving Catholics in which Catholic nuptials were held. These data are available in all chancery offices throughout the country and a reasonably accurate listing of them year by year can be found in the Catholic Directory.<sup>4</sup> There are no adequate data on the number of mixed marriages not sanctioned by Catholic nuptials. I have studied all the mixed marriages to be found in 132 parishes distributed throughout the East and Middle West. There were 29,581 mixed marriages, of which 11,710, or 39.6 per cent, were not sanctioned by Catholic nuptials. I feel that this rate is fairly representative for the section of the country covered but would hesitate to predicate the same rate of other sections of the country since my research reveals great sectional differences. However, for the purpose of testing the triple-melting-pot hypothesis, it is scarcely necessary to have accurate data on the number of mixed marriages not sanctioned by Catholic nuptials. Suffice it to say that my studies indicate there are a considerable number. Hence,

<sup>4</sup> According to canon law, only those mixed marriages which are sanctioned by Catholic nuptials are valid. All other mixed marriages involving a Catholic are invalid according to the law of the Church.

if it can be shown merely from the number of mixed marriages which are sanctioned by Catholic nuptials that the triple-melting-pot hypothesis is untenable, it can be argued *a fortiori*, that if we had complete coverage on all mixed marriages, our position would be still more firmly established.

Let us turn now to the questions we posed at the beginning of this paper. What is the mixed marriage rate for Catholics in the United States? In the first place, it should be pointed out that mixed marriage rates show extreme variations from one section of the country to another. The spread is from over 70 per cent in the dioceses of Raleigh, Charleston, and Savannah-Atlanta, to less than 10 per cent for the dioceses of El Paso, Corpus Christi, and Santa Fe.<sup>5</sup> During the decade 1940 to 1950, mixed marriages sanctioned by Catholic nuptials approximated 30 per cent of all Catholic marriages in the United States. The rate computed from the data given in the Catholic Directory for 1950 is 26.2.<sup>6</sup> Although the listing for all dioceses is not complete, this figure may be taken as a fairly accurate picture for the present period. Complete data for the thirties are not available. However, returns from approximately half the dioceses of the country for this period reveal 912,851 Catholic marriages of which 274,000, or about 30 per cent, were mixed. It seems scarcely necessary to point out that this rate of nearly one-third which includes only those mixed marriages sanctioned by Catholic nuptials, renders Hollingshead's hypothesis of "three pots boiling merrily side by side with little fusion between them"<sup>7</sup> quite untenable.

Perhaps the atypical nature of the New Haven intermarriage data can best be indicated by comparing the mixed marriage rate of New Haven Catholics with the mixed

<sup>5</sup> A diocese is a district presided over by a bishop and generally named after the city in which his residence is located. Very few dioceses cross state lines but there may be several dioceses within the same state.

<sup>6</sup> *The Official Catholic Directory, 1950*, New York: P. J. Kenedy & Sons, 1950. The rates computed from the *Directory* figures have been found to run slightly below the actual rates for most dioceses.

<sup>7</sup> Hollingshead, *op. cit.*, p. 624.

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marriage rate of Catholics living in the rest of the state. According to the figures given by Kennedy,<sup>8</sup> the percentages of Italians, Irish, and Poles, intermarrying with British-Americans, Scandinavians, Germans, and Jews, was as follows: 1870, 4.65 per cent; 1900, 14.22 per cent; 1930, 17.95 per cent; 1940, 16.29. Hollingshead, investigating all the New Haven marriages for 1948, discovered that only 6.2 per cent of the Catholics married outside their religious group.<sup>9</sup> It should be pointed out that these figures apply to all intermarriages and not merely those sanctioned by Catholic nuptials. On the other hand, in the State of Connecticut, the rate for just the mixed marriages sanctioned by Catholic nuptials was 40.2 per cent of all Catholic marriages in 1949.<sup>10</sup> If one were to complete the data on mixed marriages by adding in all those mixed marriages not sanctioned by Catholic nuptials, the most conservative estimate would place the total rate at over 50 per cent. This indicates that the city of New Haven is not representative even of the state in which it is located in regard to its intermarriage patterns.

Is intermarriage involving Catholics increasing? Complete information for many sections of the country is lacking on this point. It is known that there was a considerable increase in mixed marriages during both World War I and World War II. Some dioceses registered an increase of from five to ten per cent for mixed marriages sanctioned by Catholic nuptials alone. I have studied the records of several dioceses and have discovered a gradual but more or less constant increase from 1910 to the present. Because intermarriage rates vary so much in different sections of the country, one could venture no generalization on such an inadequate basis.

<sup>8</sup> Kennedy, *op. cit.*, p. 333.

<sup>9</sup> Hollingshead, *op. cit.*, pp. 622-623. It is possible that the discrepancy between the data presented by Kennedy and that of Hollingshead results from the fact that Kennedy seems to have studied only the major national groups, whereas Hollingshead studied all the marriages for 1948.

<sup>10</sup> *The Official Catholic Directory*, 1950, *op. cit.*, p. 379.

Let us take up our second major query: What are the principal factors determining the rate of mixed marriage in the various sections of the country. My studies reveal that there are three main factors influencing the rate of intermarriage: (1) the percentage of Catholics in the total population; (2) the presence of cohesive ethnic sub-groups; and (3) the socio-economic status of the Catholic population in the community. We shall treat these factors separately, although it may be difficult to determine their specific influence on intermarriage in any given community.

The proportion of Catholics in the total population greatly influences the intermarriage rates of Catholics. The scarcity of prospective marriage mates within one's own religious group leads to a high rate of intermarriage wherever ethnic and/or social status differences do not prevent occupational and social contacts. Although statistics on the Catholic population in the various dioceses are admittedly inadequate in many instances, there is sufficient information to substantiate our thesis. For example, in the dioceses of Raleigh, Charleston, Savannah-Atlanta, Nashville, and Little Rock, where the Catholic population is two per cent or less of the total, the mixed marriage rates are 76.3, 71.5, 70.3, 58.3, and 55.6 respectively. On the other hand, in the dioceses of El Paso, Corpus Christi, Lafayette (La.), Providence (R.I.), and Sante Fe, where the Catholic population is fifty to seventy per cent of the total, the mixed marriage rates are 8.7, 7.5, 14.2, 17.2, and 8.4 respectively. It is scarcely necessary to seek further evidence on this point but we hasten to add that the relative percentage of Catholics in the population is not the sole factor determining the rate of intermarriage as many seem to believe.

The second factor is the presence of close-knit ethnic sub-groups in the community. It has long been known that ethnic groups operate as a check on intermarriage. There are many factors combining to produce this effect—fidelity to the group, social status of the ethnic minority, religion, language, and transplanted national prejudices. The important point is that it is not religion alone



nor the relative number of prospective mates available in the religious group, which determines the intermarriage rate. For example, the percentage of Catholics in the diocese of Amarillo, Texas, is approximately 4.6. The intermarriage rate of Catholics is 26.9. The dioceses of St. Augustine (Florida), Lafayette (Indiana), Owensboro (Kentucky), St. Joseph (Missouri) all have approximately the same percentage of Catholics but their intermarriage rates are 47.2, 45.3, 46.2, 45.1, respectively. The reason for the difference in the mixed marriage rates is plain. The diocese of Amarillo has a large sub-group of Spanish and Mexican Catholics, the other dioceses do not have any prominent ethnic sub-group. One might compare the dioceses of San Antonio and Syracuse. Both have approximately the same percentage of Catholics (30.1 and 30.6 respectively). Their mixed marriage rates are quite different (13.8 and 26.8 respectively). San Antonio has an important ethnic sub-group which practices in-group marriage to a high degree.

The effect of ethnic sub-groups on intermarriage rates is even more marked if one considers individual parishes. I first arrived at my conviction of their importance while making a study of intermarriage rates in different sized cities. Drawing samples from the Great Lakes and Middle West region, I studied 25 parishes in each of the following classifications: 100,000 and over, 25,000-100,000, 5,000-25,000, and 5,000 and under. Table 1 gives the result of this study. It

TABLE 1. PERCENTAGE OF MIXED MARRIAGE  
ACCORDING TO SIZE OF CITY

Population of City	Number of Parishes	Number of Families	Percentage of Mixed Marriages
100,000 and over	25	36,353	14.9
25,000-100,000	25	15,000	24.2
5,000-25,000	25	16,624	21.4
5,000 and under	25	9,431	19.6

should be pointed out that the percentages given in the table are not the mixed marriage rates but rather the percentage of mixed marriages found in the parishes. Since there are a considerable number of mixed

marriages which end in divorce or cease to be classified as mixed marriage through the conversion of the non-Catholic party, the percentage of mixed marriages in a parish has been found to be 5 to 10 per cent below the mixed marriage rate. As Table 1 reveals, the percentage of mixed marriages found in cities of 100,000 and over was surprisingly low. The assumption had been that the rates would increase in close relation with the size of the city. What had caused the break at the 100,000 and over level? A more intensive comparative study of the individual parishes indicated that the major ethnic sub-group concentrations were in the larger cities for the territory from which we had drawn our samples. It was discovered that those parishes in the large cities in which a prominent ethnic group was located had relatively low percentages of mixed marriages. Hence, the conclusion that the presence of ethnic sub-groups in the community is an important factor in determining the rate of intermarriage.

A third factor seems to be the socio-economic class of the Catholic population. Intermarriage rates seem to be closely related to social class. This hypothesis is advanced tentatively since adequate statistical support is still lacking. However, I have made studies of several communities which furnish rather substantial evidence that the hypothesis is well founded. For example, using graded rental areas as a gauge of socio-economic status, I studied the intermarriage pattern of 51,671 families distributed in thirty parishes of a large urban center. Table 2 gives the results of this study. It should be pointed out that percentages in the table are relatively low since they

TABLE 2. RENTAL AREAS AND PERCENTAGE  
OF MIXED MARRIAGES

Rental Area	Percentage of Mixed Marriages
Lower	8.5
Mixed Lower and Middle	9.1
Middle	12.0
Mixed Middle and Upper	16.3
Upper	17.9
Suburban	19.3

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record the percentage of mixed marriages found in the parishes, not the mixed marriage rate. This study gives every indication of a rather close relation between socio-economic status and the intermarriage rate.

The answer to the third question posed at the beginning of this paper, implies a prediction. My studies lead me to believe that there will be a gradual but steady increase of marriages between Catholics and non-Catholics. My prediction is based on the following points. (1) We have seen the importance of ethnic sub-groups as checks on the intermarriage of group members. The decline in immigration, the horizontal and vertical mobility so characteristic of our population, and the increased cultural contacts facilitated by modern means of communication will make it increasingly difficult for these groups to maintain their isolation and in-group loyalties. The melting-pot is a reality although the boiling process may take longer than was at first believed necessary. (2) Mixed marriages have a cumulative effect. The children of mixed marriages tend to marry those outside their religious group more often than do the offspring of in-group marriages.<sup>11</sup> (3) The attitude of Catholic and Protestant young people toward mixed marriages seems increasingly tolerant. Landis reports a study of students' attitudes on marriage in which more than 50 per cent of over 2,000 students said that other things being equal they would marry into a different faith.<sup>12</sup> He found little difference between responses of Catholic and Protestant students. I have given the same attitude test to 224 Catholic college students in a Catholic institution and found that 33 per cent of the boys and 40 per cent of the girls would marry those of another religious group, "other things being equal." Of course, these are attitude tests and do not tell us what these young people

will actually do when the occasion arrives and parental and pastoral pressure is brought to bear on them. However, the figures are indicative of an attitude which definitely does facilitate contacts leading to intermarriage. (4) It is generally agreed that the family and the church have less control than formerly over youth. Increasing individualism and the widespread acceptance of the "romantic" view of love have tended to make the choice of marriage mates a strictly personal affair.

To summarize, therefore, this paper has tried to test the validity of the triple-melting-pot hypothesis in regard to Catholics. Although complete data on all mixed marriage are not available, it was found that a consideration of only those mixed marriages which were sanctioned by Catholic nuptials raised serious doubts concerning the value of this hypothesis. The mixed marriage rate of Catholics for the past two decades has averaged 30 per cent of all Catholic marriages. There are wide variations in rates from one section of the country and the principal factors determining these differences appear to be three: (1) the relative percentage of Catholics in the total population; (2) the presence of cohesive ethnic sub-groups in the community; and (3) the socio-economic class of the Catholic population. The prediction was made that the rate of intermarriage would go on increasing gradually but constantly for some time to come.

In conclusion, I think that the data which have been presented in this paper reveal a much higher mixed marriage rate for Catholics than the formulators of the triple-melting-pot hypothesis believed. They were, perhaps, over impressed by the low rate of intermarriage which they discovered in New Haven. As we have shown, New Haven is very atypical in this respect. It follows from our studies that religion, although important, is only one of the factors determining intermarriage rates. The single-melting-pot hypothesis is probably as valid as any hypothesis yet advanced.

<sup>11</sup> Gerald J. Schnepp, "Three Mixed Marriage Questions Answered," *Catholic World*, 156, (Nov., 1942), 203-207.

<sup>12</sup> Judson T. Landis and Mary G. Landis, *Building a Successful Marriage*, New York: Prentice-Hall, Inc., 1948, p. 138.

## AGE RELATIONSHIPS AND MARRIAGE

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THE materials I have published previously on mate selection in American society have been focused, in the main, on the relationships which obtain between social stratification and who marries whom.<sup>1</sup> The data analysed here indicate that the age and prior marital status of each partner are additional factors of crucial importance in the mate selection process.

The data used in this discussion were taken from the marriage licenses of couples married in New Haven, Connecticut during 1948 and 1949.<sup>2</sup> Four census-type items are used in the analysis: age, marital status, sex, and race. Marriage licenses provide direct data on age for individuals entering matrimony for the first time as well as for those who remarry. But in the case of individuals remarrying, only age at the time of remarriage is given. Thus, their age at first marriage is not known.<sup>3</sup> This limitation is not too serious, for 6,000 of the 7,204 individuals married in the two years under consideration (83.15 per cent) entered matrimony for the first time. The remainder married for a second, third, or fourth time. The marital relations of individuals in this remarriage group to individuals in the first-

married group give rise to some of the most important points discussed here.

When marriages are viewed from the facet of an individual's previous marital experience they fall into four main types. Each type is categorized as follows:

*Type I.* Both individuals enter matrimony for the first time.

*Type II.* The man enters matrimony for the first time; the woman has been married before.

*Type III.* The man has been married before; but the woman enters matrimony for the first time.

*Type IV.* Both individuals have been married before.<sup>4</sup>

The number of cases in each type by race, sex, and age group, is summarized in Table 1.

*Type I* marriages comprise more than three-fourths of all cases (76.1 per cent). They also include individuals whose ages were known at the time of this, their first marital experience. By age, individuals who entered *Type I* marriages fell into three groups: under 25, 25 to 34, 35 and above.

*Under 25.* Of our cases, 994 individuals were married before they were 21 years of age, and 78.5 per cent were females. Stated differently, 7.8 per cent of all males, and 28.7 per cent of all females married before they were of legal age. The difference between the percentage for each sex is highly significant.<sup>5</sup> These percentages indicate that the marriage of females in their late teens is sanctioned far more readily than that of males. They also indicate that females over age 16 but under 21 are granted permission to marry by their parents more readily than are males. This sharply different pattern,

\* "Married before" in each case means married at least once; no finer distinctions are made here. Some individuals had been married twice or three times, but for present purposes multiple remarriage cases are included with the single remarriage ones.

<sup>5</sup> The criterion of significance is statistical.

\* Grateful acknowledgment is made hereby to the Social Science Research Council for a grant-in-aid, and the Committee on Bursary Appointments, Yale University, for their support of this project.

<sup>1</sup> August B. Hollingshead, "Cultural Factors in the Selection of Marriage Mates," *American Sociological Review*, 15 (October, 1950), 619-627; *Elm-town's Youth, The Impact of Social Classes on Adolescents*, New York: Wiley, 1949, pp. 424-436; "Class Differences in Family Stability," *Annals of the American Academy of Political and Social Science*, 253 (November, 1950), 39-46.

<sup>2</sup> The marriage licenses were copied by permission of the Registrars of Vital Statistics for the State of Connecticut and the City of New Haven.

<sup>3</sup> Such an individual's age at first marriage could be determined only by direct questioning, and no interviewing was done for this phase of the study.

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TABLE 1.

Age Group

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20-24  
25-29  
30-34  
35-39  
40-44  
45-49  
50-54  
55-59  
60-64

Total

Age Group

Under 20  
20-24  
25-29  
30-34  
35-39  
40-44  
45-49  
50-54  
55-59  
60-64

Total

one would infer, is a product of the marital folkways which are more permissive for minor females than for minor males. These folkways also stipulate that a wife should be younger than her husband. Another factor in this complex is the cultural expectation that the husband should be the family provider.

The pattern of marriages between ages 16 and 21 is very different for Negroes than it is for whites. Negroes of both sexes are almost twice as likely to marry before 21 as are whites. This differential is summarized as follows:

Married Under 21	Per Cent	
	Males	Females
White	7.3	27.0
Negroes	14.7	47.4

The 994 individuals who married before they were of age did so in the face of strict legal definitions that do not recognize any differences between minor males and females and marriage. Connecticut law prohibits marriage under 16 years of age except in unusual circumstances, such as premarital pregnancy. Even then, no individual may be married without the consent of the Judge

TABLE 1. NUMBER AND PER CENT OF INDIVIDUALS OF EACH AGE, SEX, RACE, AND MARRIAGE TYPE MARRIED IN NEW HAVEN, CONNECTICUT, IN 1948 AND 1949

A. Type I Marriages (1:1)

Age Groups	Whites				Negroes			
	Males		Females		Males		Females	
	No.	Per Cent	No.	Per Cent	No.	Per Cent	No.	Per Cent
Under 20	86	3.4	419	16.5	18	9.1	81	41.1
20-24	1215	47.7	1417	55.7	99	50.3	68	34.5
25-29	819	32.2	464	18.2	47	23.9	30	15.2
30-34	265	10.4	162	6.4	18	9.1	9	4.6
35-39	95	3.7	49	1.9	5	7.6	6	4.6
40-44	37	1.5	18	.7	8		2	
45-49	18	.7	8	.3	2		1	
50-54	7	.4	5	2				
55-59	1		2					
60-64	3		2					
Total	2546	100.0	2546	100.0	197	100.0	197	100.0

B. Type II Marriages (1:2)

Age Groups	Whites				Negroes			
	Males		Females		Males		Females	
	No.	Per Cent	No.	Per Cent	No.	Per Cent	No.	Per Cent
Under 20	1	16.6	2	.9			1	14.8
20-24	37	16.6	32	14.0	3	11.1	3	25.9
25-29	55	24.0	64	27.9	11	40.7	7	22.2
30-34	52	22.7	59	25.8	4	14.8	6	14.8
35-39	32	13.9	33	14.4	5	18.6	4	
40-44	25	10.9	20	8.7	2	14.8	5	22.2
45-49	14	6.2	11	4.8	1		1	
50-54	9	5.7	5	3.5				
55-59	3		1					
60-64	1		2		1			
Total	229	100.0	229	100.0	27	100.0	27	99.9

TABLE 1. NUMBER AND PER CENT OF INDIVIDUALS OF EACH AGE, SEX, RACE, AND MARRIAGE TYPE MARRIED IN NEW HAVEN, CONNECTICUT, IN 1948 AND 1949—Continued

C. Type III Marriages (2:1)								
Age Groups	Whites				Negroes			
	Males		Females		Males		Females	
	No.	Per Cent	No.	Per Cent	No.	Per Cent	No.	Per Cent
Under 20			17	6.4			2	9.1
20-24	14	5.2	66	24.7	3	13.6	4	18.2
25-29	67	25.1	65	24.3	3	13.6	11	50.0
30-34	59	22.1	48	17.9	6	27.3	4	18.2
35-39	44	16.4	32	11.9	5	22.8		
40-44	25	9.4	18	6.7	2	9.1	1	4.5
45-49	25	9.4	12	4.6	1			
50-54	16	6.0	5		1			
55-59	8	3.0	3			13.6		
60-64	5			3.5	1			
65-69	3	3.4	1					
70 & above	1							
Total	267	100.0	267	100.0	22	100.0	22	100.0

D. Type IV Marriages (2:2)								
Age Groups	Whites				Negroes			
	Males		Females		Males		Females	
	No.	Per Cent	No.	Per Cent	No.	Per Cent	No.	Per Cent
Under 20	..	..	..	..	..	..	..	..
20-24	4	1.4	13	4.6	..	..	..	..
25-29	28	9.9	35	12.5	6	18.2	8	24.3
30-34	29	10.4	54	19.3	6	18.2	7	21.2
35-39	49	17.4	41	14.6	5	15.1	6	18.2
40-44	25	8.9	39	13.9			7	21.2
45-49	24	8.6	30	10.7	6	18.2	2	
50-54	27	9.6	31	11.0	7	21.2	3	15.1
55-59	36	12.8	15	5.3	2			
60-64	36	12.8	15	5.3	1	9.1		
65-69	12	4.3	5	2.8				
70 & above	11	3.9						
Total	281	100.0	281	100.0	33	100.0	33	100.0

of Probate as well as that of his or her parents. The law restricts marriage after the sixteenth birthday and before the twenty-first birthday has been attained to individuals who have the written, witnessed, and notarized consent of their parents. If a minor does not have a legally responsible parent, he must have a hearing before the Judge of Probate who acts in *loco parentis*. Between ages 16 and 21, no distinction is made in the marriage law between males and females; the two sexes are treated alike legally. These legal stipulations are followed

strictly in New Haven. Thus, it is rather surprising to find such large percentages of *Type I* marriages among minor females in both races. The only explanation we can offer for this phenomenon is that the folkways permit early marriage of females in significantly larger numbers than is true for males, even though the law attempts to restrict marriages, in both sexes, before an individual reaches his legal majority.

The markedly different selectivity between the percentages of males and females who marry before age 21 disappears there-

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after in the case of whites, but continues among Negroes. Between ages 21 and 24, inclusive, the percentages of white males and females who marry are almost the same, 41.1, and 41.8. Among Negroes, however, males marry almost twice as often as females; the percentages are: 49.3 for males, and 24.7 for females. These percentage differences indicate that there are two different age structures operating in *Type I* marriages in New Haven. One structure prevails among the whites and another among the Negroes. It would be interesting to see if this difference holds for other cities.

From the viewpoint of a woman's opportunities for a *Type I* marriage, the nine years that elapse between her sixteenth and twenty-fifth birthdays are the important ones. After a woman's twenty-fifth birthday, her marital choices are severely restricted unless she is willing to marry a man who has been married before. On the other hand, a man's marital choices are as good after his twenty-fifth birthday as before. By the end of the twenty-fourth year, 48.5 per cent of all males, and 69.3 per cent of all females are married. This differential is clearly a product, one would infer, of the differences in the age folkways that impinge on males and females in this culture.

*Twenty-five through 34.* By the time a woman is through her twenty-ninth year, 87.0 per cent of her age mates are married; the figure is lower for men, 79.5 per cent. After thirty years of age, there are relatively few *Type I* marriages, but the majority of males and females who have not married by their thirtieth birthday, but do marry, will do so before they are thirty-five years of age.

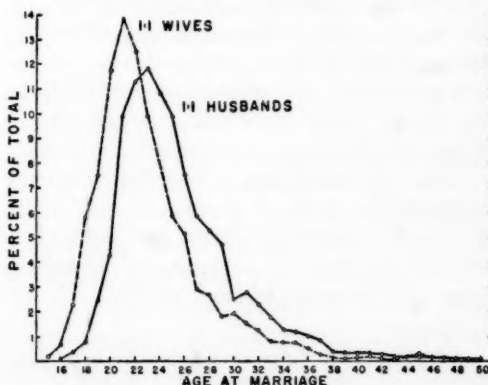
*After 35.* After a man's or woman's thirty-fifth birthday, *Type I* marital opportunities are, indeed, poor. Only 5.5 per cent of the women and 9.3 per cent of the men entered matrimony for the first time after they were thirty-five. But even in this age group the advantage is clearly on the side of a man. Perhaps, a woman above 35 does not need

the support of statistics to convince her of the force of the folkways.

The percentage of white males and females who contracted a *Type I* marriage at a given age is plotted in Chart I. The age curves in Chart I reveal that females begin to marry at 16 years of age. On the other hand, males do not marry in the same proportions until they are eighteen years of age. Percentagewise, exactly .67 per cent of the females married at age 16; among males this same percentage occurred at age 18. A study of Chart I will bring out the fact that the curve of each sex parallels that of the other with but slight variations from ages 16 through 29. From ages 30 to 45 the differences between the percentages of the two sexes who marry at a given age decline; after 45 the difference is erased.

The curves for the percentage of Negro males and females of a given age who contracted *Type I* marriages (not shown here) follow the same pattern as the curves for white males and females. However, they are higher and narrower, because Negroes marry younger and in significantly greater proportions than whites. This results from a greater concentration of marriages between ages 16 and 29 among Negroes than among whites. Another measure of the difference between the two races is the modal age at marriage. For Negro females it is 19 years; for white females 21; for Negro males it is 21 years of age and for white males 23 years of age.

CHART I. Percentage of Husbands and Wives Married at a Specific Age, Type I (Whites)

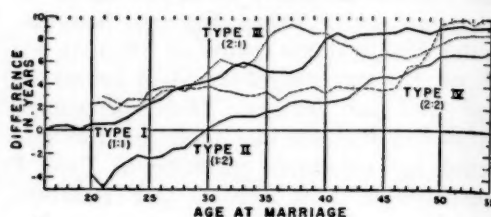


From the viewpoint of the question of the influence of legal restrictions on the marriage of minors, it is interesting to note that the modal age of marriage for Negro females is 19, two years below their legal majority. During the twenty-first year a larger percentage of Negro males and white females marry than for any other year of life, *but* the difference is not significant. Moreover, the percentage of increase is not as great between ages 20 and 21 for either sex or race, as it is between ages 17 and 18 for Negro females, and ages 18 and 19 for white females. Among white males the greatest percentage of increase comes during the twentieth year. White males show an increase in the percentage married only after age 21. In passing, it may be well to note that New Haven data are in close agreement, on this point, with what Glick has reported for the nation.<sup>6</sup>

The data plotted in Chart I indicate that in *Type I* marriages women marry in larger percentages at earlier ages than men. Nevertheless, the close parallel between the curves for the two sexes might lead to the inference that on the average women are about two years younger than men in *Type I* marriages. This inference may or may not be true. We tested it by calculating the net difference between the ages of husbands and wives. In this procedure, the age of the husband was held constant. Thus, if the husband was the same age as his wife the difference was zero; if he was one year older, the difference was recorded as a positive one; if he was one year younger the difference was recorded as a negative one. By the use of this system, the net difference between the ages of husbands and wives was determined by specific age of marriage for the husband for each marriage type. These net differences are plotted in Chart II.

Chart II shows there is little difference between the ages of husbands and wives in *Type I* marriages when the husband is married at age 19 or younger. However, when the husband marries at ages 20, 21,

CHART II. Net Differences in Years Between Husbands and Wives by Type of Marriage at Specific Age of Marriage from 16 Through 55 Years of Age (Whites)



or 22, he is some six months older than his wife. The net positive difference in the ages of husbands and wives increases to one year when the husband is 24 years of age at marriage. This difference increases until age 33 when husbands are, on the average, almost 6 years older than their wives. Then the net difference decreases by almost a year during the next five years. This means that when men contract *Type I* marriages in their late thirties, they pick wives from 5 to 8 years younger than themselves. After age 40, men entering their first marriage on the average select women who are approximately a decade younger than they are. Thus, for men who marry at 40 for the first time, the average age of their wives is approximately 30; for men who marry at 55, the average age of the wives is 46 to 47.

The age difference for *Type II* marriages is the exact opposite of the other three types when the husband marries under thirty years of age. Moreover, the younger the husband, the greater the negative difference between himself and his wife in a *Type II* marriage. This difference is greatest when the man is 21 years of age; in such a case, the woman averages 26 years of age. This negative differential may be explained by the fact that the woman has experienced one marriage cycle; and enough time had to elapse for her to be widowed or divorced and to select another mate. Two unsolved points of interest are: (1) what kind of men do they marry; or (2) do these women have talents which enable them to marry men younger than themselves? Another point about *Type II* marriages is that after

<sup>6</sup> Paul C. Glick, "Age as a Factor in Marriage," *American Sociological Review*, 15 (August 1950), 517-529.

age 30 they have the least age differential between husbands and wives. Women who have been married before and marry single men appear to select husbands either younger than themselves or near their own age when they remarry in significantly greater numbers than do women in the other marital types.

The age difference curve for *Type III* marriages closely parallels *Types I* and *IV* until age 29; after age 30 the curves for *Types I, III* and *IV* diverge only to come together again at 50. The curve for *Type IV* exhibits the least variation from ages 20 through 46; then between ages 46 and 50 the difference between the age of the husband and the wife almost doubles. After age 50, *Type IV* husbands tend to marry women ten years younger than themselves.

The data on the net differences between the ages of husbands and wives give point to the widely held belief that a man ought to be older than his wife. On the other hand, there is another folk belief frequently encountered in New Haven, and elsewhere, which states, "A young woman should not marry an old man;" the reverse is "Don't rob the cradle." Such folk sayings are elements in the complex of cultural compulsiveness which give rise to the behavior plotted on Chart II.

The data on net differences between the ages of husbands and wives indicated the presence of a strong positive association. The magnitude of this association was determined by the calculation of coefficients of correlation for each marriage type in the two racial groups. The association was very high in each type and race. Likewise, there was variation in the magnitude of the coefficients for each race and marriage type as a glance at Table 2 will show. But this variation was not significant at the five per cent level of probability. We consider this an important finding, and we have made two inferences on the basis of it. First, the folkways of our culture limit the marital choices of a man or woman of a particular age and marital status to individuals of the opposite sex in the same approximate age range. Second, these folk-

TABLE II. CORRELATION BETWEEN THE AGES OF HUSBAND AND WIVES AT TIME OF MARRIAGE BY TYPE OF MARRIAGE AND BY RACE

Marriage Type	r=	
	Whites	Negroes
I. (First for Husband and Wife)	.74	.69
II. (First for Husband, Second <sup>1</sup> for Wife)	.64	.73
III. (Second for Husband, First for Wife)	.78	.84
IV. (Second for Husband, Second for Wife)	.77	.84

<sup>1</sup> Second in each case includes all cases that have been married at least once before.

ways are binding on all marital types and on both races to the same extent.

The structure of the age groups involved in the different marriage types was analyzed by the use of means, variances, and medians. The null hypothesis that there is no essential difference in the age structure of one marriage type when compared with a comparable one was rejected, when we found that the age structure of each marital type was significantly different from a comparable one. For example, the age structure of single males who contracted a *Type I* marriage was significantly different from single males who contracted a *Type II* marriage. The *Type III* males were not comparable to the *Type IV* males in their age structure as measured by means and variances, but both types were remarrying. Likewise, when the *Type I* females were compared with the *Type II* females, and the *Type III's* with the *Type IV's*, the age structure of each type was significantly different from the comparable one. The same rule held for both whites and Negroes.

The differences in the age structure of each marriage type, as measured by median age at marriage, are summarized in Table 3. A study of Table 3 discloses marked differences in the median age of marriage for each sex and race when comparisons are made between the pure and mixed types. For example, the median age of marriage for white males in *Type I* was 24, but the median age of white males in *Type II* was

TABLE III. MEDIAN AGE\* AT MARRIAGE BY MARRIAGE TYPE, SEX, AND RACE

Marriage Type	Males		Females	
	Whites	Negroes	Whites	Negroes
I (1:1)	24	24	22	21
II (1:2)	33	29	33	33
III (2:1)	34	34	29	26
IV (2:2)	45	38	42	36

\* No correction has been made for the fact that on the average each person was 6 months older than the marriage record indicated.

33. In both instances, the males were married for the first time, but the males who entered *Type I* marriages were 9 years younger, on the average, than their fellows who married women who were remarrying (*Type II*). Viewed from the other end of the age structure, men who entered *Type IV* marriages were 9 years older than those who entered *Type III*. In this type the man marries a woman who also has been married before. However, the *Type III* man, although he has been married before, marries a woman who enters matrimony for the first time. A close study of the figures in Table 3 will show a marked difference in the median age of marriage for each sex and race when the data are viewed in terms of the type of marriage that is involved. In no instance are the medians less than five years different for comparable groups, and in the case of the *Type III* and *Type IV* males there is an eleven-year differential. These differences indicate that there is a selective factor at work which sifts individuals of different ages into different marital categories.

The effects of marital status on the age structure is even more striking when the

divorced and widowed cases are separated in *Type IV*. For example, the median age of divorced men, in *Type IV* marriages, was 35 years, but for widowers it was 60 years. The median age of the wives of *Type IV* divorced men was 33; of the *Type IV* widowers it was 52. The same pattern was exhibited by the *Type II* and *III* marriages. In each instance divorced individuals were several years younger than widowed ones, and the median age of the persons they married was, correspondingly, lower or higher. The exact medians for each type of combination are presented in Table 4. On the basis of significance tests<sup>7</sup> the conclusion is reached that the marital status of the spouse is a powerful influence in the age structure of each marriage type. Furthermore, one may infer that a combination of marital status with age is a very important determinant in who marries whom.

As we analyzed our data the idea occurred to us that marriage and remarriage are functions of age. This idea was tested by the calculation of the percentage of individuals who married or remarried at a specific age. The results of this calculation are plotted in Chart III. A close study of the marriage and remarriage curves in Chart III will show that the percentage of individuals first married falls rapidly after age 23, and the percentage of remarriages increases in a complementary manner. The two curves cross at age 43, when there are

<sup>7</sup> The F Ratio was used to measure significance, and the 1 per cent level was relied upon. Also for each test of significance between means a correction was made for the difference in variances where applicable.

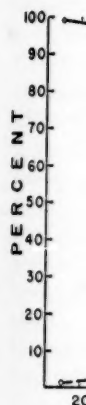
TABLE IV. MEDIAN AGES FOR DIFFERENT COMBINATIONS OF MARITAL STATUS (WHITES)

Marital Combination		Median Age		No. of Cases
		Males	Females	
Type II,	Wife Divorced	30	29	177
Type II,	Wife Widowed	35	35	52
Type III,	Husband Divorced	33	27	207
Type III,	Husband Widowed	42	37	60
Type IV,	Both Divorced	35	33	116
Type IV,	Both Widowed	60	52	73
Type IV,	Both Divorced or Widowed	47	42	92

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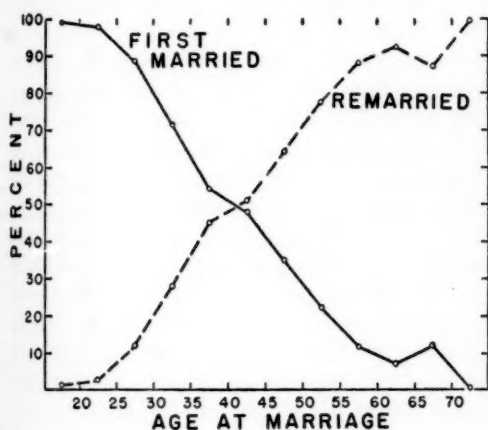
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as many remarriages as first marriages. From this point on the two curves diverge consistently, with one fluctuation in the middle sixties. These curves demonstrate dramatically how closely marriage and remarriage are linked to age.

*Summary and Conclusions.* This paper has analyzed certain relationships that exist between the ages and marital statuses of spouses. Age and marital status are vital factors in each marriage for a marriage of necessity involves two persons who possess both an age and a marital status. Before marriage their age and marital statuses have

CHART III. Per Cent of Individuals, Males and Females, who Married and Remarried, by Age, in New Haven during 1948 and 1949



a direct bearing on their probabilities and opportunities for marriage with persons of the opposite sex. The significant relationships we have found between age and marital status may be summarized in the following propositions:

1. The marital choices of males are limited, in large part, to females their own age, or a few years younger; whereas, the marital choices of females are channelized toward men their own age or a few years older than themselves.
2. Individuals who contract marriages with individuals of a different marital status than themselves are significantly different in age from individuals who marry in their own marital status.
3. Marriage and remarriage are functions of age.

These three propositions, bulwarked by a large body of statistical data, throw new light upon the mate selection process. The direct relationships between age and marital type indicate that in future studies of the question of age in marriage, researchers should analyze their data in terms of marriage type if the most fruitful results are to be attained. Finally, we believe it is fair to suggest that the Census Bureau might find this idea of value in its studies of age and marriage in the national population.

## SPEECH AND SOCIETY: A SOCIOLOGICAL APPROACH TO LANGUAGE

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Colombo, Ceylon

### SPEECH-COMMUNITIES

THE sociologist's interest in language is due primarily to its social effects, its role in sociation or dis-sociation. A common language is an important symbol of social solidarity. The original meaning of the Latin term *barbaros* was probably stammering, stuttering, babbling unintelli-

gibly, and other peoples had similar derogatory epithets, such as the Slav word for German which means mute, dumb.<sup>1</sup> But if a language can unify a group vis-à-vis foreigners or out-groups, it can equally reflect differentiation and divisions of interest

<sup>1</sup> F. Hertz, *Nationality in History and Politics*, London, 1944, p. 78.

within a society. A complex official or formal group like the state or nation contains innumerable informal sub-groups which cultivate minute linguistic differences. Runyon has familiarized us with the slang of the American racketeer or "spiv." The scientific fraternity has its own distinctive speechways; a cynical diarist has left an account of a meeting of the Entomological Society, addressed by a man of "very considerable scientific attainments," in a jargon which his fellow-scientists had come to regard "as symbols of a ritual which they think it pious to accept without question."<sup>2</sup> Similarly, local dialects arise in different parts of a country—the broad accents of the Yorkshireman differ from the Londoner's quaint "cockney."

Speechways are also indices of social distance between different classes of a complex society. In *Great Expectations* the sensitive and impressionable Pip was humiliated by Estella's slighting remarks: "He calls the knaves, Jacks, this boy! . . . And what coarse hands he has! And what thick boots!" Speech and dress distinguished this "common laboring-boy," and with incomparable artistry Dickens traces Pip's reaction to a harrowing experience when he is later made to confide, "I want to be a gentleman. . . . I am not at all happy as I am. I am disgusted with my calling and with my life." In Western Europe today the dress of different social strata varies but little.<sup>3</sup> The hallmark of the London "cockney" is his distinctive mode of speech. The "old school tie" may well be a hackneyed music-hall joke, but a misplaced "h" is sufficient to betray a man's breeding, his education, his social class. A social class is not demarcated by legal decree, although even in England the law does on occasion take cognizance of gentility, and "what are called high spirits in university students on Boat Race nights

becomes serious misconduct as we move east of Temple Bar."<sup>4</sup> Intra-social *rapprochement* is the pullulation of behavior-patterns cultivated by persons having common interests or approximately equal statuses. In other words, certain behavior-patterns give rise to classes having a kind of *esprit de corps*. If there is what Sombart calls a "specifically proletarian psyche," it is the creature of social vehicles such as language. Words like "comrade" have a type of linguistic use which Malinowski labels *phatic communion* whereby "ties of union are created by a mere exchange of words."<sup>5</sup>

#### LANGUAGE, ETHOS, AND PERSONALITY STRUCTURE

In view of the importance of language as a socializing vehicle, speech has rightly been regarded as a personality trait. Of the dominant conditioned responses which give rise to relative permanence in the structure of an individual's personality, the symbolic, especially the verbal, is of vital importance. A society dominated by scientific thoughtways demands a language capable of a high degree of precision and abstraction. Malinowski has pointed out that the Trobriand Islanders' practical interest in nature and society extends to their speechways. They refer to all plants for which they have no economic use by the single collective word "bush." In contrast, European botanical classification includes all known flora, irrespective of economic utility. Such an attitude is the product of a scientific ethos. For, as Max Weber has pointed out, even the choice of objects for scientific investigation and the extent of penetration into the infinite causal web are determined by the values dominating the mental climate of the investigator. Speechways reflect thoughtways; there is a reciprocal interaction between the two. Hence the language of a pre-scientific society does not permit the ready expression

<sup>2</sup> W. N. P. Barbellion, *The Journal of a Disappointed Man*, London, 1919.

<sup>3</sup> In England the dress of the high and the low, even among women, tends to converge. The working girl's apparel is almost identical with that of the Royal Princesses. (J. Laver, *Fashion and Class Distinction*, Pilot Papers, I, 1945.)

<sup>4</sup> H. J. Laski, *Studies in Law and Politics*, London, 1932.

<sup>5</sup> B. Malinowski, *The Problem of Meaning in Primitive Languages*, Supplement in Odgen & Richards, *The Meaning of Meaning*, London, 1943.

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of causal sequences. Ignorance of physiological paternity makes it almost impossible for Christian missionaries to convey to the tribesmen of New Guinea the Christian dogmas of God the Father and God the Son, the sacrifice of the only Son, the filial love felt by man for his Maker, and so on. Malinowski's contention is that "all this falls somewhat flat in a matrilineal society, where the relation between father and son is decreed by tribal law to be that of two strangers, where all personal unity between them is denied."<sup>6</sup> The language of the Trobrianders expresses subsequence rather than consequence or cause. This lack of cause-and-effect modes of thought conditions their speechways. There are no clear-cut temporal categories in Trobriand speechways, and the tenses are ambiguously symbolized. Hence their lack of a sense of history.<sup>7</sup>

Words therefore are instruments for the expression of life-experiences, vehicles for the communication of ideas and for the transmission of adult cultural traditions to children. Speech itself grows out of human interaction, a verbal utterance being meaningless except in its *context of situation*. Speechways then are symbolic of the mores of a society. In other words, the language of a society mirrors the situational imperatives of that society. Thus Evans-Pritchard contended that any argument which would utterly demolish the validity of Zande claims for the power of the Oracle, when translated into Zande modes of thought via Zande language, reappears as a complete justification for their entire system of oracular beliefs.<sup>8</sup> Likewise West<sup>9</sup> has shown how the process of translation of the English sen-

tence, "You must know that I am thinking of his marrying one of them," into Bengali by Indian students invariably reflected the difference between English and Indian marriage customs. The given sentence was rendered in translation as "I am thinking of marrying one of my girls to him," the underlying idea being the Bengali custom for parents to arrange suitable marriages for their children on caste lines, the ideal of "falling in love" and romantic love being alien to their thoughtways.<sup>10</sup> Similarly the polite euphemism "going to bed with so-and-so" can be current only in a society in which sexual intercourse is performed in a reclining posture. But many people are bedless, and the practices of certain Oriental societies are evidence of different modes of approach. And different expressions, less inhibited, are current.<sup>11</sup>

<sup>10</sup> Expressions such as "head-over-heels in love" have little significance for people who conceive of marriage as an act of rational calculation. In the closely-knit village-communities of the Orient, and in Central Europe, the marriage broker was a central figure. He is portrayed in Smetana's Czech opera "The Bartered Bride" and in modern Ceylonese comedies in a satirical spirit, a protest against the old order. Erich Fromm traces the emergence of the European ideal of romantic love to the post-Renaissance loosening of the primary affective bonds of family and community which characterized feudal society. The consequent insecurity experienced by the emancipated individual led him to seek refuge in the emotive personification of some miraculous power. "This process of personification of the Magic Helper is to be observed frequently in what is called 'falling in love.'" (Fromm, *The Fear of Freedom*, London, 1942, p. 150.) The apotheosis of idealized love after the French Revolution was misnamed Platonic; in Greek society this was homosexuality. In modern China, revolt against the parental generation gave rise to the European conception of love and marriage as rights to be earned, a manifestation of the Westernized adolescent's guilt, anxiety, and insecurity caused by emancipation from affective communal bondage. (A. S. Chin, "Some Problems of Chinese Youth in Transition," *American Journal of Sociology*, 54; 1948). The novel is a barometer of changing attitudes to love.

<sup>11</sup> Modern prudery heavily censors the commonest English colloquialism for the sexual act. It can appear only furtively in print, in underworld literature, and in banned books (cf. the Stockholm edition of D. H. Lawrence's *Lady Chatterly's Lover*.) Likewise the "shame" attached to defecation is evidenced in the *sotto voce* use of the

<sup>6</sup> B. Malinowski, *The Father in Primitive Psychology*, London, 1927, p. 56.

<sup>7</sup> Cf. D. D. Lee, *A Primitive System of Values*, Philosophy of Science, Vol. 7, 1940. Also S. S. Newman, "Personal Symbolization in Language-Patterns," *Psychiatry*, Vol. 2, 1939.

<sup>8</sup> E. E. Evans-Pritchard, *Witchcraft, Oracles and Magic among the Azande*, Oxford, 1937, pp. 319-320.

<sup>9</sup> M. West, *Bilingualism (with special reference to Bengal)*, Calcutta, Bureau of Education, Government of India, 1926.



The stages in the linguistic development of the child is usually parallel to the gradual maturation of his personality. For a whole months after birth an infant cannot even recognize another person. "The child's self develops as his communication takes on an objective reference to others."<sup>12</sup> The differentiation of the infantile ego from "the generalized other" is only partly the product of rational thought, largely the consequence of friction, antagonism, and pain involved in the individual's relations with the social and physical world. And the formation of the "I" is accelerated, the independence of the ego-structure fortified, as the child acquires a more effective mastery of language.

"The 'I' is the individuality in the psychological-social sense."<sup>13</sup> But the development of individuality differs from society to society. In a static community in which sociality and mutuality govern the relations between man and man, individuality is imperfectly developed. Linguistic forms mirror the relatively undeveloped ego in the ambiguous first person singular pronoun. In some American Indian languages the collective form of "I" is ordinarily used ("I-amongst-others" or "We"), and the corresponding selective form, "I-for-one," is employed only exceptionally. There is reason to believe that the collective "we" precedes the "I" in linguistic evolution. In feudal societies the plural pronominal forms are frequently used in this way, and a vestige of this is seen in the Royal "we." "Here

nursery term for faeces, caca. Freud suggests that this inhibition of a natural function in childhood results in sublimation of anal erotism and is instrumental in structuring a personality marked by excessive parsimony and obstinacy—traits which are frequently transferred to financial matters in later life. (See S. Freud, "Character and Anal Erotism," and "Civilized" Sex Morality and Modern Nervousness,"—two celebrated papers in his *Collected Papers*, Vol. 2, London, 1908.)

<sup>12</sup> K. Young, *Personality and Problems of Adjustment*, London, 1947, p. 204. Also G. H. Mead, *Mind, Self, and Society*, Chicago, 1934; M. M. Lewis, *Language in Society*, London, 1947; and E. Sapir, "Speech as a Personality Trait," *American Journal of Sociology*, 33 (1927).

<sup>13</sup> Muzaffer Sherif and H. Cantril, *The Psychology of Ego-Involvements*, New York, 1947.

we seem to have group-consciousness holding its own against individual self-consciousness, as being for primitive folk on the whole the more normal attitude of mind," concludes Marrett.<sup>14</sup>

#### SOCIAL STRUCTURE, SPEECHWAYS, AND THOUGHTWAYS

Relative freedom from instinctual determination of behavior is the prerogative of man alone; it is the cultural imperatives that condition the behavior of *Homo sapiens*. The extent of this cultural determination of thought and action differs, however, from society to society. Let us first consider the role of language in static societies which are highly stratified.

In traditional Ceylon, for example, speechways vividly reflected the rigid status-roles of a feudal society. Caste was the keystone of the social structure. The king chose his provincial governors from the nobility (the *goiwanse*, or farmer caste), although people of low castes may have surpassed them in riches. "But it is the Birth and Parentage that innobleth," commented Knox long ago.<sup>15</sup> Thus women had at least twelve titles, used according to rank; men had parallel designations. There were several words for Thou or You, and they were applied to people according to gentility—Knox lists "To, Topi, Umba, Umbela, Tomnai, Toms, Tomsela, Tomaxi," one being higher than the preceding. It is interesting to note here the use of the plural forms Topi, Umbela, Thamusela (Knox's Tomsela), for the singular "you" to indicate heightened respect as compared to the singular forms To, Umba, Thamusé. Every verb could also be rendered according to the respect to be accorded to the person addressed, e.g., "go" could be politely expressed "yanda," a request, or "pala," a command, equivalent perhaps to the English "get away," or, if accented

<sup>14</sup> R. R. Marrett, *Anthropology*, London, 1912, p. 146.

<sup>15</sup> Robert Knox, *An Historical Relation of Ceylon*, published in 1681 with an "Advertisement" by Sir Christopher Wren and a Preface by Robert Hooke (Glasgow edition, 1911). This classic work is practically a monumental ethnographic monograph, written after 20 years' residence.



appropriately, "scram!" These forms were provided in the language of the masses, Sinhalese, which is still spoken by over 90% of the Sinhalese population.<sup>16</sup> But linguistic differentiation went further. As Ribeiro put it, "They have a language different from the one in common use, just as Latin among us; only their chief men learn it, and they are of subtle intellect."<sup>17</sup> It was "a learned and dead language—the Pali, derived from and said to be similar to Sanskrit."<sup>18</sup> There are parallel cases of linguistic bifurcation elsewhere. In Italy, the thirteenth century "curiale" was employed by the courts and the poets, while the masses had their diverse local dialects.<sup>19</sup> In England, the "courtly" Latin and French was the language of the literati at different periods; Newton's *Principia* and several works of Milton and Hobbes appeared in Latin, and previous to that, in 1298, Robert of Gloucester said, "For unless a man knows

French, he is held of little account." Again, in Java *Noko* is the language of the nobles, while the commoners speak *krómo*, but the two classes understand each other's language and each uses the other's tongue in conversation.<sup>20</sup> Not so in feudal Ceylon; "the language is too high for me" was a frequent apology of a commoner interpreting a conversation of a priest or chief.<sup>21</sup>

In such a society, as we would expect, "their language is Copious, Smooth, Elegant, Courtly; according as the People that speak it are. Who are full of Words, Titles, and Compliments" (Knox). Some 150 years later Dr. Davy commented on the Sinhalese fondness for intricacies of style, the more artificial, the more admired: "I have heard a poem spoken of with delight as an extraordinary effort of genius, the particular merit of which was, that it admitted a great variety of readings, from the left to the right, up and down, and in many other ways, each making sense." Style here was cultivated for the sake of style. But grandiloquence and elaborate literary stylization masks decay; it is symptomatic of artistic stasis. For to stylize is to derealize, to dehumanize, and artistic stylization of this type is symbolic of an ethos which loathes living forms and attempts to elaborate abstract and dehumanized media of artistic expression.<sup>22</sup> In Ceylon this stylization was the outcome of Buddhism, its intense asceticism being emphasized in the twelfth century when the Church was "purged of heresy" by the expurgation of ritualistic and mystical Hindu corruptions, and the country reverted to the pure form of Buddhism known as the Little Vehicle (Hinyana), which it retains even today. Characteristically, this phase marks the end of the Heroic Age. "Decadence is always archaistic," says Hocart; "The revolt wears itself out: the energy departs; the monstrous ceases to be vigorous and is merely tame,

<sup>16</sup> Even after almost five centuries of European rule, and the use of English as the official language by the native elite for some 150 years, the traditional speechways are persistently used in the vernacular according to caste—an indication of the tardiness of popular acceptance of a bleak individualistic, though "democratic" ethos. The formal or "literal" democracy of adult franchise is a poor substitute for the sacrifice of communal solidarity and family unity, from the point of view of the masses still living in feudal village-communities. There have, however, been instances of titular vulgarization, e.g., *rala hamy*, originally prince, refers to a police constable today; *Kumarihamy*, which designated noble ladies is arrogated by all Kandyan women (cf. A. M. Hocart, in *Man*, XXXVII, 1938). The nationalist agitation for a revival of Sinhalese as the official language has, significantly, been inaugurated by the English-educated native elite, an indication of their insecurity in an individualistic order. It is part of a general desire to return to the past, which Toynbee has described as Archaism, and is symptomatic of a "schism in the soul" of the marginal man (cf. "Sinhalese and Tamil as Official Languages," *Report of a select Committee of the State Council, Sessional paper XXII, 1943*; also H. A. Passe, *The English Language in Ceylon*, London University Ph.D. thesis, Mss., 1948.)

<sup>17</sup> Ribeiro, *The Historic Tragedy of the Island of Ceilao*, Lisbon, 1685, translated by P. E. Pieris, Colombo, 1948.

<sup>18</sup> John Davy, *An Account of the Interior of Ceylon*, London, 1821.

<sup>19</sup> J. C. Burckhardt, *The Civilization of the Renaissance in Italy*, translated by S. G. C. Middlemore, London, 1945.

<sup>20</sup> E. D. Chapple and C. S. Coon, *Principles of Anthropology*, London, 1947.

<sup>21</sup> Davy, *loc. cit.*

<sup>22</sup> Cf. the brilliant analysis in Ortega Y Gasset, *The Dehumanization of Art, and Notes on the Novel*, Princeton, 1948, also G. Grigson, "How Much Me Your Acrobatics Amaze," *Polemic* 3, 1946.

and nothing is left but that standardized and uninspired art which is the only Indian art known to Europeans."<sup>23</sup>

It is interesting to compare the modern European reaction to living forms. Its keynote is the avoidance of the naturalistic literary and artistic forms of the last century. Stylization in Asia was confined within bounds by a mental climate steeped in archaism. European art-forms are not hemmed in by convention. In their dehumanization of living beings they are unhampered by archaism. The speechways inaugurated by James Joyce are typical of the literary aspect of the modern movement in art. Such literature employs a verbal symbolism private to the author; it revels in neologisms and word-play; and, above all, it is anti-popular, and can be appreciated only by a literary elite.<sup>24</sup>

#### SPEECHWAYS AND SOCIAL CHANGE

We have seen that social differentiation in an immobile society finds expression in its speechways, particularly in its elaborately gradated terms of deference. Convention and purism marred the freshness and vigor of many an able writer in Italy at the time of the Renaissance, said Burckhardt; others were tempted to rely on harmony and flow rather than on content. But soon there was a struggle to establish an universal language capable of clarity and simplicity of expression. In Europe, the transition from a feudal to an individualistic order, from status to contract, from *gemeinschaft* to *gesellschaft*, had its parallel in linguistic changes.

<sup>23</sup> A. M. Hocart, "Decadence in India," in *Essays to C. G. Seligman*, London, 1934.

<sup>24</sup> Thus James Joyce ends an account of an opera singer: "Hats off, primi assoluti! Send him canorous, long to lung over us, high topseasoarious . . ." ("From a banned writer to a banned singer" in *Turnstile One*, ed. by V. S. Pritchett, London, 1948.) It is well to note, however, that even the speechways of Joyce are not purely random. The vocabulary of his *Ulysses* follows a hyperbolic rank-frequency word distribution, and so in common with others, he is subject to the "Principle of Least Effort" (G. K. Zipf, *Human Behavior and the Principle of Least Effort*, Cambridge, Massachusetts, 1949.)

Now individualism as it exists in contemporary Europe and America is a comparatively recent phenomenon. Mediaeval European society lacked this extensive individual freedom. In fact, the individual was practically chained to his social role. The social order was conceived of as a natural universe to which the individual was by duty bound: "Man was conscious of himself only as a member of a race, people, party, family, or corporation—only through some general category," said Burckhardt. This mediaeval veil, woven of faith and religion, melted in the Renaissance, and "man became a spiritual *individual*."<sup>25</sup> This individualization and democratization was reflected in changing speechways.

Forms of address which symbolized manorial overlordship and feudal status-roles were depreciated in an individualistic society which substituted the cash-nexus for feudal status. Thus "master" (Fr. *maitre*, Ger. *Meister*), deriving from a designation expressing lordship, in its modern form "mister" is applied to all persons superior to manual laborers. "Your obedient servant" and "yours faithfully" were feudal realities; "yours" literally meant ownership of slaves by their "masters." Today these expressions are conventionalized. Lordship further connoted superhuman attributes. The Anglo-Saxon origin of Lord is *Balder*, the name of a favorite son of Odin, a God. In Germany *Mein Herr* was a phrase applied by a serf to his lord, a symbol of his servility (serfility). Today it is familiarly applied to all and sundry. *Sire* was apparently originally applied only to the highest; *Sir*, a derivation of *Sire*, originally meant King. The female counterpart of *Sir* and *Monsieur* were likewise originally terms of adoration. *Dame* originally meant high born. *Ma Dame*, was contracted to *madam*, or the slipshod *ma'am*. "We find that 'Yes'm' of Sally to her mistress is originally equivalent to 'Yes, my exalted,' or 'Yes, your highness,'" writes Herbert Spencer.<sup>26</sup>

<sup>25</sup> J. C. Burckhardt, *op. cit.*

<sup>26</sup> See H. Spencer, "On Manners and Fashion," *Westminster Review*, April, 1854, for a number of these examples.

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Vulgarization of titles and forms of address is symbolic of a dissolution of the social bonds and status-relations they represented. Such titular devaluation indicates that former status-roles are no longer accorded deference or honor. Bestowal of honor and respectful titles signifies a stratified society, but plurality of honorable titles depreciates their worth. Thus *Lord*, when applied to the descendants of a charismatic leader is devalued by virtue of its traditional or routinized application to a numerous posterity, many of whom are devoid of the original ancestor's "gift of grace." It finally degenerated into an epithet applied to all powerful feudal barons. In the same way, individualized or personal honor was a polemic concept which served the middle classes in their struggle to overthrow the feudal conception of honor and status. These middle classes, struggling for recognition in the social scheme, succeeded in making the soul and the mind rather than mere "empty" titles and manners the sources of honor. "The conspicuously honourable behaviour of the nobility was devaluated to mere gestures, irrelevant politeness, symbols of an insignificant order, against which was set up a realm of 'natural' inner quality accessible to everyone alike."<sup>27</sup> This struggle had its expression in linguistic transformations. To take one more example, the expression *escuyer* originally denoted mediaeval knightly orders, feudal military chivalry. Ultimately it degenerated to the modern *esquire* which describes every one below the status of a knight.

It is evident from the foregoing discussion that language in general, and forms of address in particular, change vis-à-vis social changes. As Max Muller put it, "A struggle for life is constantly going on amongst the words and grammatical forms in each language. The better, the shorter, the easier forms are constantly gaining the upper hand, and they owe their success to their inherent value."<sup>28</sup> In a static society

the impetus for linguistic innovation is slight. Hence the "classical" fossil-languages like Latin and Sanskrit are unadapted for modern use. In societies in process of substantial change, ever new speechways add to the richness and range of expression of a language. We have seen that vulgarization of titles was symbolic of a democratic transformation. It was the linguistic counterpart of a widespread erosion of status-relations of mediaeval society. There was less punctiliousness displayed in the removal of the hat as a mark of respect—it was merely touched, not raised. Prostration at the feet of a superior gave way to a low bow which was successively replaced by a slight inclination of the body, and finally by a mere offhand nod. The scant attention paid in England to details of etiquette, such as raising the hat, has been coupled by Herbert Spencer with the fact that the English are the freest (i.e. the most individualistic, democratic) nation in Europe.<sup>29</sup> This freedom is also manifest in the devaluation of feudal forms of address. Sumner has shrewdly commented on the advent of slang in America: "A people who are prosperous and happy, optimistic and progressive, will produce much slang. It is a case of play. They amuse themselves with the language."<sup>30</sup> Unsullied by tradition they evolve what is in effect a new language—the American language. It is in this way that hybrid languages arise. French was the corruption of Latin; Creole spoken in Mauritius is in turn a corrupt French. In a society acutely conscious of its cultural traditions, on the other hand, emphasis by purists on standard or "correct" English makes the language more rigid and formal than in a society without these inhibitions.

favored words in the struggle for existence is natural selection.

<sup>29</sup> Herbert Spencer, *op. cit.* This essay was written almost 100 years ago.

<sup>30</sup> W. G. Sumner, *Folkways*, Boston, 1907. Contrast this linguistic flexibility with the recent refusal by the English parliament to standardize spelling by decree. The extent to which language can be changed by decree is problematical. The Turkish script was successfully transformed.

<sup>27</sup> Hans Speier, "Honor and Social Structure," *Social Research*, 2 (Feb., 1935).

<sup>28</sup> Darwin, quoting this in *The Descent of Man*, concludes that preservation or survival of certain



## THE CONCEPT OF JEWISH CULTURE AND THE STATE OF ISRAEL\*

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It is usually not difficult to define culture demographically, to relate a specific culture to a given group of people. By definition, Roman culture is that of the Romans, French that of the French, American that of the Americans, and so on. True enough, even in these as in all cases of this kind, the correspondence between a given group of people and their culture is never complete. As soon as contacts are established between different cultures—which is as far back as our records go—some people born into one culture may find attractive the offerings of another. This preference may extend to any aspect of the other culture, to its values, to the manner in which it implements them, or to the way in which it tends to satisfy certain needs. Some Englishmen may prefer French literature and French cooking to their own; some Frenchmen may be attracted by American technical ingenuity; while some Americans would like to acquire the social graces of the French. Those who feel attracted by another culture may attempt to modify their own in its image, to emulate it, or to exchange altogether their own for the other. Such exchange is, however, only rarely wholly intentional. It is usually a concomitant of population shifts. Migrations, if they are of a relatively free kind, appear to be prompted mainly by material needs. They normally result in what is called acculturation, or acquisition of the main traits of the foster-culture. Such more or less voluntary flow of populations from one culture-area to another has been quite extensive during the last century or so, and has come to a virtual halt only because of enforced restrictions. In itself, migration

of this kind was not only deemed to be in no way objectionable, but in some cases, as for instance in Ireland and in countries of Eastern Europe, it was even actually encouraged.

### THE ODIUM OF DEFECTION

Jews, like any other people, could of course not fail to experience the attraction of other cultures. From the time when some of them had acquired a strong taste for the "fleshpots" of Egypt to the present when many of their descendants have attachment for the high living standards of America they, like most other human beings, were strongly motivated by material needs. What distinguished them decisively from other peoples was, however, the stigma they attached to the surrender of their own for the sake of another culture. This was true so long as they were in possession of a country of their own, but dispersion only intensified the odium attached to defection. A Jew who decided to exchange his own for another culture could not hope to be able to do so and keep peace with his own people. He was not merely given up as lost; his conversion was considered as an act of moral turpitude, befouling himself and bringing shame to his relations and friends. Even today, no observant Jewish community will want to have any truck with an estranged person whom it condemns as apostate. The sharpness of moral and social condemnation of defection appears thus to be one of the most distinctive features of Jewish culture. It offers a clue—by the way largely neglected—to the understanding of the specific character of this culture, and stresses at the same time the difficulty of assessing it in terms generally applicable to other cultures.

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## A DIVERSITY OF SUB-CULTURES

This difficulty extends even to the basic, the demographic definition of Jewish culture. Looking at the situation today, what we see is a number of ecologically disjointed smaller or larger groups of people called Jewish. For close to two thousand years now, these groups have lacked any unified systems of political, economic, or social institutions. Wherever they exist, Jews do so as more or less underprivileged minorities. For the sheer sake of self-preservation they are forced to adopt many of the customs and mores, the habits and fashions of the majority from whom they are often undistinguishable in outward appearance, mental attitudes, and conduct of life. An American Jew is likely to think, feel, and act more like an American than as a Jew, an English Jew like an Englishman, an Italian Jew like an Italian, and so on. If there is any personal contact between these "Jews of different nationalities," it will tend to be colored more by their respective economic or social status than by their Jewishness. Some of these groups, particularly those in the Anglo-Saxon countries, have been on the whole favored by circumstances. Those in Eastern European countries lived mostly in abject poverty, exposed to continuous and often murderous persecution, until total disaster befell them. It would seem that the only link between all these groups was identity of faith and isolated strains of folklore related to it. Even here, there was unity only on a very abstract level. The concrete manner of worship showed a great range of variety. What similarity is there between a "shul" in Poland, where the men wear "shtraymls" and the women "shaytls,"<sup>1</sup> and where both are seated separated from each other by a curtain—and a palatial synagogue in New York where the men

are not allowed to keep their hats on, and where virtually all prayers are said in English?

In view of all this, it might not be quite unjustified to ask whether there exists at all anything which all these groups can claim to have in common. And even if so, whether its nature is of the kind we are wont to designate as culture. If there is a Jewish culture, its unifying strength must by far surpass that of other cultures, for no other culture has to create unity out of such diversity of sub-cultures. Whether there is such unity and, if so, what it consists of, is an open question. To have any weight, an answer will have to be based on a multiple kind of research. What Jewish culture is can be established only from a thorough insight into what its sub-cultures are.

## THE STATE OF ISRAEL, A PUZZLING FACTOR

Paradoxically enough, this task has been made rather more difficult by the establishment of the state of Israel. As long as Jewish group existence was clearly dominated by religion, Jewish culture could be simply identified in terms of the supremacy of specific religious values and their implementation. It was only when nationality superseded religion that identification became the problem it is today. This process of substitution was accomplished by Zionism. This movement, for the first time since the Diaspora, succeeded in uniting for concerted action sizable parts of all the dispersed groups, and brought forth such central economic and political institutions as the Zionist Congress, the National and the Foundation Fund, and the Jewish Agency. The aims of this movement were from the beginning so markedly national that it was and is still being opposed by those Jews who—like the extremely orthodox, or the "American citizens of Jewish faith"—for one reason or another continue to insist on purely religious identification. The establishment of the state of Israel has stamped the Zionist aspirations with the seal of accomplishment. For those who personally identify themselves with the new state, either by being or by intending to

<sup>1</sup> These expressions are Yiddish. A "shul" means a house of learning and is used as an equivalent for synagogue in English; a "shtrayml" is the elaborate headdress, consisting of a round black velvet cap framed by brown fur and fur-tails, worn on Sabbath and Holidays; a "shaytl" is a wig worn by orthodox Jewish women whose own hair is shorn at the wedding ceremony.

become its citizens, the problem of Jewish culture has been apparently solved beyond dispute. There seem to be, however, many, particularly in the Anglo-Saxon countries—and among them not a few of its active promoters—who find themselves strangely affected by this historic accomplishment. Judging from their first reactions, it seems as if they were inclined to consider Israel as just another of the many possible kinds of Jewish settlements. The fact that it enjoys all the privileges of political autonomy adds, they seem to feel, merely a new strain to an already puzzling variety and one apt to make the existence of and the adherence to a Jewish culture more rather than less problematic. Some of them seem to be disposed to solve the problem drastically by arguing that exactly because of the existence of a Jewish state there is no longer any justification for calling anybody Jewish who is not its citizen. Consequently, and with all sympathy for the new state, they seem to see no reason why they should not consider themselves as fully and unequivocally American as those whose ancestors happened to be citizens of say England, Germany, Ireland, or any other country. Should this trend gain in momentum, the numerical extent of defection would by far surpass the size of the possible demographic reintegration in the new state. By winning its case against assimilation, Zionism would thus actually have lost it. A truly paradoxical effect of accomplishment.

#### TWO KINDS OF ASSIMILATION

We may find this effect less puzzling if we consider closer the nature of assimilation. Park and Burgess, in their *Introduction to the Science of Sociology*, defined assimilation as "a process of interpenetration and fusion in which persons and groups acquire the memories, sentiments and attitudes of other persons or groups, and, by sharing their experience and history, are incorporated with them in a common cultural life" (2nd edition, pp. 735-736). Generally speaking, assimilation, then, is a process of interaction. It can take place between two or more individuals or between two or more groups.

In both cases it results, as the term implies, in likeness. In the case of individuals, the likeness refers to personal, in the case of groups to cultural, traits. Implying mutuality, the process will affect both sides. The modification will be by far stronger on the part of the traits deemed to be inferior, and much lesser on the part of those which present themselves as superior. In other words, assimilation may result in some slight, if any, change on the one side, and in such complete alteration on the other that it may well amount to a loss of the original cultural identity.

It is from the opposition against such loss of Jewish cultural identity that Zionism drew one of its strongest appeals. This was probably because, as we have seen, sharpness of moral and social condemnation of defection is one of the most distinctive traits of Jewish group existence. While thus indicting individual assimilation, however, the Zionists at the same time emphatically advocated emancipation, calling on the Jewish people to reject their shameful and inferior ways of life,<sup>2</sup> and to become a nation like other nations. The Zionist program did not set as its goal the kind of state which would, as of old, owe its existence to the grace of God, but rather a National Home based on the public recognition of the Powers of the World. It was not the unique mono-theocracy of old which was to serve as a model for its form of government, but the parliamentary system of the modern national state. In short, while condemning assimilation, the Zionist program proposed a Jewish state similar to other states in all their virtues and vices.

To argue, however, from this seeming contradiction that what is right for the Jewish state is right for the individual Jew, is by no means correct. Such argument is based on faulty distinction. We are dealing here with two kinds of assimilation, one of individuals, the other of a group. Their respective effect may be and usually is, op-

<sup>2</sup> See the invectives quoted by Abraham I. Golomb from Ezekiel Kaufman in "Jewish Self-Hatred," *Yivo Annual of Jewish Social Science*, New York, 1946, Vol. I. p. 251.

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posite. The one tends to weaken, the other to strengthen group coherence. In a historic situation which seemed to offer only the two alternatives of either group preservation and ghetto, or assimilation and disintegration, Zionism demonstrated the possibility of yet another choice. It showed that integration can be made compatible with assimilation when assimilation is turned into a controlled group enterprise.<sup>3</sup>

#### THE QUESTION OF GROUP COHERENCE AND JEWISH CULTURE

The success of this demonstration would have produced a less perplexing reaction if the enterprise could have involved all or at least a substantial part of all the Jews living in dispersion. As it is, the state of Israel can accommodate only a small part of the total Jewish world population. Those who prefer, or are forced by circumstances, to remain in the Diaspora find that the existence of the new state actually aggravates their own situation. It gives rise to formerly non-existent issues, such as that of divided loyalty, and lends new urgency to the question of group coherence and Jewish culture. The issue of double loyalty may possibly be dismissed as forced. The question of group coherence and Jewish culture does not admit of such easy solution. On the manner in which it is solved will probably depend the fate of Jewish identity outside of Israel.

Unfortunately, little help can apparently be expected in this connection from the Jewish state itself. Political rehabilitation has not been paralleled by a resurgence of a common Jewish culture. It is true that even before the establishment of the state, the Jewish settlement in Palestine took marked interest in the arts and sciences. The fine arts, music, literature and the drama were cultivated to an extent far out of proportion to the size of the population. All the efforts,

however, have failed, at least so far, to produce any new commanding cultural pattern. Virtually all the creative minds of Israel, with the possible exception of those whose field is the science of Judaism, have been shaped by alien influences. Their work, even if its subject matter is Jewish, lacks specific Jewish quality. It has not yet become exceptional to the point where it could command attention on the part of the critical outsider, nor so authoritative as to claim allegiance on the part of the non-Israeli Jew. The revival of the Hebrew language is undoubtedly a great cultural feat. But it, too, affects mainly those who live in Israel. The knowledge of Hebrew remains, even among the Zionists, so limited that it can hardly serve as a means of communication between them and Israel. The only surpassing attainment of national rehabilitation appears to lie not in the field of fine arts or the sciences, but rather in the realm of socio-economic organization. The modern cooperative community, the Kvitza and all its related forms of communal existence, is the one creation of Israel which has won international acclaim and admiration. In many countries today it serves as a model for rural rehabilitation and agricultural reform. As far as the Jews outside Israel, and particularly in America, are concerned, the Kvitza, however, had hardly any effect on their thinking or on their existence, and conditions being what they are, it is difficult to see how it could. It is interesting to note, by the way, that the Kibbutz-movement itself has experienced, since or, as some think, because of the establishment of the state, difficulties which to some appear to assume the proportions of a crisis.

#### THE NEED FOR SCIENTIFIC ANALYSIS

Possibly because of the emulative character of the achieved political autonomy, the Jewish state has not succeeded in establishing the kind of cultural authority from which the Jews in other countries could derive strength in the face of their present perplexities. In this respect the situation is particularly acute for the Jews in America, a country whose democratic ideology tends

<sup>3</sup> In a study of Poles and Germans in Dutchess County it was similarly found that the Poles—by making assimilation a controlled group enterprise—succeeded in turning it into a process of cultural group integration. See this author's "The Aged in the Process of Ethnic Assimilation," *Sociometry*, 3 (1940), 358 ff.



to put, at least in theory, a premium on assimilation. The conditions here are such that it seems to make less sense to ask what compels Jews to remain Jews than to wonder what it is that keeps them from becoming Gentiles. This complexity of the situation has in recent years stimulated among Jewish leaders a great deal of interest in the social sciences. Symptomatic of this interest is the work of the Commission on Community Interrelations of the American Jewish Congress, the Department for Scientific Study of Anti-Semitism of the American Jewish Committee, the comprehensive symposium on Jewish Social Research in America conducted by YIVO and published in its Annual of 1949, a number of searching articles in Jewish magazines, and last, but possibly not least, the recent attempt to establish an independent Association for the Advancement of Jewish Social Research, in New York City. All these efforts no doubt are bound to produce a great deal of clarification. They seem to leave aside, however, questions which ought to be of primary importance. These questions, related to the general problem of Jewish culture today, could be formulated as follows: Is Jewish cultural autonomy possible outside of Israel? If so, how can we circumscribe its nature and its scope? And if not, what is to take its place in making Jewish group coherence possible? Or, applied more specifically to America which in many respects holds today a key position, what kind of cultural autonomy is here possible? And, if possible, is it desirable? If, as is evident, only a certain degree of autonomy can be maintained, where is the line to be drawn? By what criterion are to be distinguished those values which must be preserved from those that may be dispensed with? Finally, what role is to be allotted in the determination of these values to Jewish groups in other countries, and to the state of Israel? If, on the other hand, no kind of Jewish cultural autonomy is possible, are there any other common causes which can give sense to continuous Jewish group existence in America? If so, what are these causes? If no such causes exist, is the

alternative complete assimilation? And if so, is it feasible?

Answers to any of these crucial questions will prove of little avail if based on obscurantism or wishful thinking, on impressionistic opinion or the exigencies of fund raising. To substantiate them will require more than statistical compilation or conventional community studies, as useful and even indispensable as such undertakings might be in a preliminary way. What will be needed is, it would seem, an approach oriented by theory which lends itself to empirical verification. If possible, this theory should be capable of yielding objective methods of fact-finding and devices of standardized observation apt to make research cumulative. What will be needed is, in short, a sound scientific procedure which, as far as the social sciences are concerned, is still largely indicative of their level of aspiration rather than of achievement.

#### KURT LEWIN'S THEORY OF GROUP-DYNAMICS

It is interesting to note that what is probably the most significant attempt to close this gap between aspiration and achievement has been made partly in connection with problems of Jewish group-existence. We have in mind the studies in group-dynamics and particularly on "inter-group conflicts and group belongingness" by the late Kurt Lewin. The central concept in Lewin's theoretical framework is the group seen as a "sociological whole" with dynamic properties of its own, such as organization, stability, and goals which, however, are "something different from the organization, the stability, and the goals of the individual in it."<sup>4</sup>

It is Lewin's claim that this definition "takes mysticism out of the group conception and brings the problem down to a thoroughly empirical and testable basis."<sup>5</sup> His specific contribution lies in the attempt to demonstrate that the behavior of individ-

<sup>4</sup> This and the following quotation are from the posthumously published book by Kurt Lewin: *Resolving Social Conflicts*, New York. Harpers, 1948, pp. 73, 74.

<sup>5</sup> Kurt Lewin, *op. cit.*, New York, 1948, p. 87.

<sup>6</sup> Kurt Lewin, *op. cit.*, p. 87.

<sup>7</sup> I remember showing me unauthorized in Gestalt psychology.



uals as well as of groups, dependent as it is on their situation in general and their peculiar position in it, can be described in scientific and even mathematical terms. The mathematical tool which Lewin employed for this purpose was that of "topology," the geometry of spatial relations, based, as he explained, "on the relationship between 'part' and 'whole' or in other words on the concept of 'being-included-in.'"<sup>5</sup> It is with the help of this tool that he thought it possible to determine "in a geometrically precise manner, the position, direction, and distance within the life-space, even in such cases where the position of the person and the direction of his actions are not physical but social in nature."<sup>6</sup>

The most valuable result of this attempt lies, it would seem, not so much in its mathematical implications—which Lewin himself later on tended to minimize—but in the sharp and methodologically fertile distinction of the sociological from the psychological point of view. It was this differentiation which cleared the ground for the famous Iowa experiments with authoritarian, laissez-faire, and democratic group atmospheres. These experiments are still being looked upon as exemplary instances of a controlled genuinely sociological experiment.

#### THE PROBLEM OF "DOUBLE UNCERTAINTY OF BELONGING"

Lewin, who always had been keenly aware of his Jewish heritage,<sup>7</sup> came to America as a refugee from Nazi Germany. It was natural for him to become concerned with the issues facing American Jewry. He saw its problems partly as those common to all underprivileged minorities, partly as specific to the situation in which the Jews as a group find themselves here. Characteristic of this situation he found to be what might be

called the double uncertainty of belonging which so largely determines the behavior of many American Jews. "It is," he stated, "one of the greatest theoretical and practical difficulties of the Jewish problem that Jewish people are often, in a high degree, uncertain of their relation to the Jewish group. They are uncertain whether they actually belong to this group, in what respect they belong to this group, and in what degree."<sup>8</sup> This problem is, however, complicated by the fact that they are also uncertain about their belonging to the majority group with which they enter into numerous contacts. The tension this double uncertainty creates becomes most acute where the boundary between minority group and superior majority grows so dim as to become virtually invisible, as for instance in the case of Jewish youth from wealthy families.<sup>9</sup>

The most desirable way to resolve this tension would obviously seem to be that of shedding all Jewish associations and becoming one of the majority. For many, the only obstacle to this solution lies in the fact that the majority as a whole shows not only no willingness of condoning such "crossing of the line" but actually a strong resistance against it. In Lewin's terminology this is the "barrier" which separates the underprivileged minority from the majority in a way as to narrow the "space of its free movement." The American Jew, caught at this barrier and not feeling like moving back to the center of his own group, but never fully admitted "over the line," develops all the symptoms characteristic of the "marginal man."

The remedy proposed by Lewin is of a twofold nature. As far as the individual is concerned, he may seek a solution in a "realistic understanding of the sociological facts." These facts find their expression in that "interdependence of fate" which defines the Jews as a group. It is this sense of interdependence that can provide the "firm social ground" which the individual

<sup>5</sup> Kurt Lewin, *Principles of Topological Psychology*, New York: McGraw-Hill Book Co., 1936, p. 87.

<sup>6</sup> Kurt Lewin, *Resolving Social Conflicts*, p. 74.

<sup>7</sup> I remember the amused pride with which he showed me a Yiddish magazine with the—of course unauthorized—translation of one of his first studies in Gestaltpsychology.

<sup>8</sup> *Ibid.*, p. 148.

<sup>9</sup> Cf. Meyer Greenberg: "The Jewish Student at Yale: His Attitude Toward Judaism," *YIVO Annual*, New York, 1946, vol. I.

Jew will have to gain if he ever wants to overcome his "marginality" and to replace it by a sense of security.<sup>10</sup> For the Jewish group as a whole, Lewin recommends as one of the "possible sources of strength" that it take a "long-range view which includes the past and the future of Jewish life and link its own minority problem with that of the welfare of all human beings."<sup>11</sup>

It is, of course, hardly possible to present here more than the gist of Lewin's approach. Enough might have been said, though, to provide a hint at the merits as well as the shortcomings of his method. To state it briefly, it has the general merit of applying to concrete issues consistent systematic concepts which lend themselves to empirical and possibly even experimental verification. It has, specifically, the merit of breaking the ground for social research which would be both scientifically sound and ethically meaningful in the sense that what he termed "action-research" was the kind of fact-finding needed to determine action that is right.

The shortcomings of Lewin's approach are, in general, those that seem to be common to natural science methods when applied to socio-cultural phenomena. Even though Lewin's field theory "accords" better than other objective methods of interpretation with the dynamic properties of human group existence, it is not altogether free from their common failing which is an inability to take fully into account the specific human capacity for rational choice and deliberate action. This probably is the reason for the relative vagueness of the terms of his proposed solution which, like "interdependence of fate," oddly recalls that mysticism he claimed to have eliminated from group conception. This tendency to underrate the human capacity for self-determination explains also the relative neglect of the by no means rare instances of deliberate and often heroic loyalty to the Jewish group, motivated in past history by religious, and in recent times by national, faith. There are indications that

Lewin was aware of the importance of this specifically human factor, as for instance when he speaks of the "degree to which fulfillment of the individual's own needs is furthered or hampered by his membership in the group" as being an "important factor for the strength of the forces toward and away from the group."<sup>12</sup> What interests him, even in this connection, however, is more the effect this satisfaction or frustration has on the "atmosphere, structure, and organization" of the group than on the meaning it holds for the individual and his capacity for choice between alternative courses of action. To account for this meaning and this capacity is the task for a comprehensive theory of human social existence. Structural concepts form, no doubt, a significant part of such a theory. As far as this part is concerned, Lewin's contribution is of great value. As for the rest, it is the very incompleteness of his theory which makes further systematic advance at the same time possible and necessary.

#### A TASK FOR JEWISH SOCIAL RESEARCH

It would seem that the first step in this direction would have to be a sociological definition of culture. Such a definition would have to specify the functional interrelation between the mode of interaction, or as Lewin would call it the "structural configuration of socio-dynamic properties," and both the aggregate of acquired meanings on the one side as well as the needs of individuals on the other. In this sense, it could be possibly formulated as follows: Culture is an acquired aggregate of meanings attached to and implemented in material and non-material objects which decisively influence the manner in which human beings tend to interact so as to satisfy their needs.<sup>13</sup>

<sup>12</sup> *Ibid.*, p. 191.

<sup>13</sup> It is extremely interesting to note that one of the most important recent anthropological studies, G. P. Murdock's *Social Structure* (New York, Macmillan, 1949), which has been hailed as a major advance in social science, adopts such a "functional" interpretation of culture, for which the author acknowledges his debt to his teacher in sociology, A. G. Keller, (p. xii).

<sup>10</sup> Kurt Lewin, *op. cit.*, pp. 165 ff.

<sup>11</sup> *Ibid.*, p. 199.

By "aggregate of acquired meanings" we understand something equivalent to what constitutes culture in the eyes of anthropology. The "whole of material and non-material values together with the vehicles of their implementation," as anthropology likes to define it, is a somewhat static complex. By substituting for values the term "meanings" we at once open the possibility of relating the cultural element to what interests the sociologist most: the mode of sociation. In this way, a place is also accorded to that factor which the natural science point of view tends to neglect, the active element in human nature. Acquired meanings are both those accumulated and transmitted by former generations, the social heritage, as well as those which the present generation makes actively its own, the cultural activities of the present. In this manner, the nature of the acquired meanings has a direct functional relation to the mode of social interaction. In its turn, the mode of social interaction is functionally related to and oriented toward the satisfaction of needs of the interacting individuals. Actually, like any true functional interrelation, the one presented in our definition can be analyzed by starting from any of its terms. Taking its starting point, for instance, from the acquired meanings, the analysis can show how, by way of the mode of social interaction, they affect the nature of the needs. Or, by starting from the needs—taking them generally as being of the kind that can be satisfied by acting mainly for oneself or of the kind that can be satisfied by acting mainly together with others—it can be shown how they influence the mode of social interaction which in turn determines the selection, acceptance, and cultivation of specific meanings attached to material and non-material objects. Finally, the analysis can set out from the mode of social interaction and show how this interaction forms,

so to speak, a relay system between meanings and needs. Wherever we start from, it is clear that the sociologically relevant character of a given group's culture can be understood fully only if the analysis is capable of accounting not only for the main terms of the culture but for the functional interrelation of these terms as well.

In the light of an analysis of this proposed kind, the many questions raised with regard to Jewish culture in America can be reduced to three systematically coordinated and functionally interrelated questions such as: (1) Since the meanings attached to material objects are for Jews largely the same as for non-Jews, are there any meanings attached to non-material objects which, by being specifically Jewish, are capable of satisfying the needs of Jews better than any other such meanings? (2) Are the ways in which these meanings determine the manner of Jewish group existence better apt to satisfy the needs of Jews than ways determined by other meanings? And finally, (3) Do Jews have needs that can be satisfied better than in any other way by specifically Jewish meanings and group existence?

It can be seen that the answer to any of these three questions calls for facts which belong to the domain of each of the three main social science disciplines respectively, cultural anthropology, sociology, and social psychology. By combining these facts, we should be able to find out what we need to know most today, namely, What is the present stand of Jewish culture? In what way and to what degree does it satisfy any primary needs? And in what way and to what degree does it have to be re-defined and re-acquired in order to do so better? To find out this is a proposition that can be handled neither easily nor quickly; but it is one, it would seem, that Jewish social research can ill afford to neglect or to put off.



# SOCIALIZATION, LOGICAL REASONING, AND CONCEPT DEVELOPMENT IN THE CHILD

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THIS is a second paper dealing with the development of children's concepts of money. In the first paper a group of business-class children was discussed.<sup>1</sup> Evidence showed that both boys and girls consistently pass through stages of learning, and that each stage is based upon knowledge characteristic of previous stages. Descriptions of behavior at each level were given, and certain limits which age places upon the attainment of levels were noted. In the present paper emphasis is on the following points:

(1) The performances of a group of working-class children (Fairview School) and those of the business-class group (University School)<sup>2</sup> are substantially the same. This suggests that in regard to certain simpler elements of perceiving and role playing there are no important social class differences.

(2) Methodological problems involved in the application of scale analysis to a complex concept, such as money, are considered in further detail.

(3) Detailed consideration is given to the problems of whether conceptual development is cumulative, and whether to reach any given developmental stage the child must have passed through all preceding and simpler stages. If we are to understand socialization in children, viewed partially as conceptual learning, the delineation of consecutive stages is at least a preliminary task.

(4) Stages of conceptual development were previously interpreted by means of such terms as simple-complex, concrete-abstract, rigid-flexible, egocentric-relativistic. This interpretation is supplemented by an analysis

of stages in terms of a logical notation. This analysis contradicts the widely held hypotheses that (a) children reason like adults except they make more mistakes due to false premises based on inadequate experience, and (b) the motives and perceptions of children are essentially the same as those of adults.

## STAGES OF CONCEPTUAL DEVELOPMENT

A main hypothesis tested in this study was that a child's concept of money develops in a consistent and cumulative manner. As in the previous study, three simple aspects of this concept were singled out for investigation: coin recognition (naming coins by various criteria); comparative value (which money is worth more); and equivalence (making change).<sup>3</sup> Three schedules were employed to bring out features of concept development, and the scalogram was used to determine whether this development was cumulative.

*Scaling and Class Differences.* Like the business-class, the responses of the working-class children to each of the three schedules formed a scale, as judged by the reproducibility coefficients, all of which exceeded 90 per cent.<sup>4</sup> This means that from a child's rank order in the group it is possible to predict with at least 90 per cent accuracy his response to every item in the schedule.

The degree of reproducibility for each of the three tests was about the same for both school groups. However, the number of scale

<sup>3</sup> Tests 1, 2, and 3 will refer respectively to these three aspects.

<sup>4</sup> An area can be scalable if the error of reproducibility does not exceed 10 per cent. Reproducibility coefficients were based on second approximations. Identical second approximation scoring weights were used for both groups so that error differences could not be ascribed to the use of different weights.

<sup>1</sup> "A Study of Concept Learning by Scale Analysis," *American Sociological Review*, 15 (Dec., 1950), 752-762.

<sup>2</sup> Both groups were described previously.



types for each test was slightly different. These differences result from a difference in the number and position of cutting points in the score arrays; which difference in turn is probably a consequence of sampling errors. For example, the maximum number of scale types<sup>5</sup> will not be reached if one response category has a zero frequency, since in that case the number of cutting points is reduced by one. It was for this reason that on the test of comparative value the Fairview group fell short of the maximum by one, hence had one less type than the University group who reached the maximum. No Fairview child gave the "best" answer to the fifth item: evidently, this item had no differentiating value for this school group. The number of scale types will also fall short of the maximum if the cutting points for two or more items fall at the same place in the array. On the test of recognition the University group fell short of the maximum 11 by 2 because three cutting points fell at the same point in the array, automatically resulting in the loss of two scale types.

Also, scale types unique to each school group occur at the same rank in the array and have small frequencies. Scale types with small frequencies are unstable since by moving the cutting point one or two places in either direction the results of the scale analysis can be changed, although not significantly. By moving the cutting point down one place on Item 5, Test 1, an additional scale type for the University group is created, reducing the discrepancies between the two school groups by one.

The foregoing analysis supports the view expressed above that differences between scalograms for the two social class groups are a sampling effect. Minor differences in the number, composition and rank order of the scale types would undoubtedly diminish, if not vanish, if comparisons were made between large samples. From our standpoint the striking feature of the comparison is not

the differences, which may be reasonably ascribed to sampling fluctuations, but the very similar progression of both groups.

The middle- and lower-class children were also contrasted in regard to mean test scores, with age controlled by matching. Mean scores on equivalence and comparative value were not significantly different; but the difference between mean scores on recognition was significant at the 1 per cent level. Since the scale types of both groups on recognition were almost identical, as noted above, the difference between means necessarily is due to a difference in relative frequency with which each type occurred for the two groups.

Inasmuch as there were no major differences between the developmental patterns of the two groups of children, it was advantageous to scale their responses together. When this was done the three topics scaled by the criterion of 90 per cent reproducibility. These results rather than the separate scalograms are used in the following discussions, since they have more stability and show better the distinctive features of each scale type—such as relative frequency, characteristic error, organization of response, and median age. This material is presented in Tables 1 and 2.<sup>6</sup>

*Sex and Age Differences.* In the lives of American men and women, monetary matters are differently emphasized; for instance the adult male does most of the investing while his wife is more likely to be expert at finding bargains. However, whenever there is no difference between the behavior of males and females, one could contend that these facets of their activities fall outside the sex roles. This appears to be so with the behavior covered by our tests. Though one might expect that boys and girls would differ somewhat in their conceptions of money, there are no significant differences between sexes concerning the rather simple knowledge of money involved in recognizing

<sup>5</sup> The maximum number of scale types is obtained by subtracting the number of items from the sum of the categories and adding unity. Thus, the maximum number of scale types for Test 1 is  $18-8+1=11$ .

<sup>6</sup> Scale types on equivalence items have been published in table form for the University children. The scalogram for both groups combined differed only in two inconsequential details from the scalogram for the University group. Age average and range were virtually identical.

TABLE 1. CHARACTERISTICS OF SCALE TYPES BASED ON RECOGNITION ITEMS

Scale Type	Response Level	Interpretation and Typical Error	Age Range (in months)	Median Age
1	Distinguishes button from coins. Knows only the penny; thinks all silver coins are nickels.	Discrimination of penny and nickel based on color and/or size. Confusion of nickel and other silver based on color criterion.	54-70	60
2	Knows that three nickels are all nickels but cannot state why.	Size and/or color.	54-96	74
3	Recognizes some of the foreign coins.	Makes correct responses because foreign coins happen to resemble U.S. coins, especially the penny and nickel. No rationale for responses beyond "it looks like one" or "it just is."	73-86	85
4	Recognizes all U.S. coins.	Discriminates through color and size. Fails on unfamiliar foreign coins which have no U.S. counterparts: e.g., a large copper coin is "a penny" or "don't know."	62-124	89
5	Recognizes all foreign coins where value is given by number.	This recognition is accomplished by reading the number inscription on each coin. Child still fails to realize that value may be written: e.g., the British half-penny "looks like a big penny," "is a half-dollar," or "don't know."	66-144	98
6	Is able to identify British half-penny, to state why three nickels have same value.	Recognizes that sometimes value is inscribed on coin by letter, but fails to realize the importance of these inscriptions.	93-130	128
7	Can point to letter inscription of value on all U.S. coins.	Recognizes regularly that value is inscribed by lettering, but still fails to grasp its importance: e.g., dime changed to color of penny would be a penny.	96-135	109
8	Understands that the value of a coin does not depend on its color. Can state why three nickels have same value.	Still does not thoroughly understand the importance of coin's inscription: dime changed to color of penny would be a penny.	92-171	112
9	Understands that the value of a coin does not depend on its size.	Inscription is crucial index of value. Doesn't know, however, that foreign coins have different names than U.S. coins; five francs is five cents, the "francs" being disregarded or meaning "France."	92-156	117
10	Identifies the five franc piece.	Understands that foreign coins may have different names than U.S. coins, but does not understand that the different name may indicate a value not expressed in terms of dollars and cents: e.g., five "francs" is five "cents."	121-143	125

coins and in being able to find equivalences and comparative values. Like the University children, Fairview boys and girls, matched by age, were similar in their performances as judged by the results of the *t* test: no *t* value reached the 5 per cent level of significance.

A high correlation between chronological age and performance was found for business-

class children. Fairview results are about the same.<sup>7</sup>

Also the same limitations placed by age upon level of conceptual development are

	Fairview	University
$r_1$	.81	.85
$r_2$	.69	.68
$r_3$	.95	.87

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TABLE 2. CHARACTERISTICS OF SCALE TYPES BASED ON COMPARATIVE VALUE ITEMS

Scale Type	Response Level	Interpretation and Typical Error	Age Range (in months)	Median Age
1	A few correct choices in pairs of two coins and in pairs of one and several.	Correct responses through rote memory, size of coin, or "whim." Typical rationale: "It's bigger."	58-83	60
2	Several incorrect responses in pairs of two coins.	Correct responses through rote memory or size of coin. No comprehension of comparative value: e.g., a pile of four pennies is worth more than a nickel because there are more pennies.	60-86	67
3	No incorrect choices in pairs of two coins.	Rote memory and/or rudimentary grasp of value. Fail to understand comparative value of piles of coins: e.g., pile with quarter is worth more because of the quarter.	54-110	78
4	A few correct choices in pairs of several coins.	Guessing or rudimentary grasp of value. Typical error as in level below; or nickels miscounted as dimes and vice versa.	54-100	78
5	No incorrect choices in pairs of one and several coins.	Understanding of simple comparative value, but not of complex value. Typical error: child has difficulty in "holding in mind" his count after he has finished counting a coin.	75-144	97
6	No incorrect choices in pairs of several coins.	Understanding of complex value. But understanding is unstable, for when coins are not actually present, size can obscure value: e.g., the big box of coins is worth more than the little box of coins merely because the box is "bigger."	84-171	115
7	Understands why smaller box may have more money than a larger one.	Understanding of complex comparative value is quite stable. Fails to grasp relation between value and metal.	67-151	117
8	Understands that some metals are worth more than others.	Understanding without recognition that value is related to rarity. Incorrect rationales are given for different values of metals.	108	108

found again in the Fairview data. These limitations can be seen from Tables 1 and 2 where median age and range are given for each scale type. Statements about the earliest age at which a child can be expected to fall into a given scale type cannot be made with great certainty because of limited sampling; nevertheless the figures strongly suggest that definite limitations are placed upon acquisition of monetary concepts by the factors subsumed under the general term "age."

*Perception and error.* In recent years the topics of perception and error have been

treated largely in terms of "need" and "interest." By way of contrast, we wish to emphasize the point, not that need and interest are unrelated to perceiving, but that they affect perception only after concepts are learned. That is, the complex perceptions of the adult rest upon his possession of certain complex concepts. Before the child acquires these concepts, his perceptions are relatively undifferentiated.

Tables 1 and 2 bear upon this point. Specifically, they support the hypotheses that (a) perception is organized around concepts and (b) that error is in part due to the

subject's conceptual deficiencies.<sup>8</sup> At each stage of development, the child exhibits blindness and commits errors because he has not yet grasped necessary conceptual distinctions. A detailed and lengthy analysis of error could be made but we shall confine ourselves to a few illustrations.

A child may have learned to perceive—i.e., to distinguish between—the various coins, but he may not yet perceive that these coins have writings on them, or may consider these writings irrelevant. The inscriptions may also lead him into what the adult would consider contradictory positions. One child stoutly maintained that a Canadian penny was not a penny when the maple leaf was showing but that it was a penny when the head was face up. The child may see two nickels not as members of a single class in the logical sense—and therefore interchangeable—but consider them as two different objects. One is his own, and the other is someone else's, and they cannot be substituted one for the other. In sociological terms, he has a sacred attitude toward his own nickel rather than a secular.

The child may make errors of addition as well as omission. Thus at the time when they learn that a nickel "is five cents," but as yet have no concepts of these classes in the adult sense, the children sometimes believe the nickel actually contains the pennies. If you were to open up the nickel, you would find the pennies therein—sometimes brown, sometimes white.

A little knowledge can be dangerous: whereas some children choose as more valuable any pile containing a large coin, some children who have become sophisticated about number choose, say, two nickels and a dime over a penny and a fifty-cent piece "because this one has three and that one has two." Similarly, whereas very young children ignore inscriptions and so think

Canadian nickels are just nickels, older children who cannot yet read the number inscription but who notice the figures will balk when confronted with the Canadian nickel, which has a beaver on it.

The very youngest children fail to match exactly the investigator's coins when told to do so, for they see merely that he has put forth some things, or they see a number of coins rather than specific coins. The investigator, also, may show children of all ages who fail to make change abstractly how to do so. The child may say he understands (i.e., "sees"). Actually he often does not, since his imitation proves to be faulty.

Interesting cases of reversion or confusion sometimes occur. Thus one child took the Portuguese ten-centavos coin for a penny, because it is brown. When the 10 was pointed out to him, he decided immediately that it was a dime. Later, in the test of equivalence, the investigator included this coin with the others, leaving the 10 clearly in view. The child then sometimes treated it as a penny and sometimes as a dime. Similarly, a child was supposed to match the investigator's thirty cents, and the only way he could do this was to give from his own pile five pennies, three nickels, and a dime. After several trials he announced that it could not be done. The reason it could not be done was that the child treated all the pennies in his pile (nine) as a single unbreakable unit instead of regarding each as separate. In logical terminology, he did not treat each penny as a class representative, equal and equivalent to every other representative—something he was previously able to do when encountering simpler problems, and was still able to do when his mistake was pointed out.

#### ASSOCIATION

To determine how conceptual development in one area was related to development in the other two, (a) correlations between test scores were computed and (b) association between specific items on tests was gauged by  $\chi^2$ .

The correlation coefficients indicated that general levels of conceptual development are

<sup>8</sup> Cf. A. Lindesmith and A. Strauss, *Social Psychology*, pp. 69-74; A. Luria, "The Problem of the Cultural Behavior of the Child," *Journal of Genetic Psychology*, 35 (1928), 493-504; and such works of J. Piaget as *La Formation Du Symbole Chez L'Enfant*, 1945, *Le Développement Des Quantités Chez L'Enfant*, 1941, and *The Child's Conception of the World*, 1929.

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directly related. This had been previously established for the University group.<sup>9</sup>

To measure association between specific items in different tests, responses were arranged in contingency tables and  $\chi^2$  values computed when possible. This detailed analysis confirmed what had been anticipated from the correlation results. The least amount of association, judged by the relative number of times  $\chi^2$  reached the 1 per cent level of significance, was between recognition and evaluation; the association was about equal between recognition-equivalence and evaluation-equivalence. Further, this statistical material shows that, for any pair of items, success tends to be related to success. An important exception, brought out by the contingency table, is that while success on a simple item can be predicted from success on a difficult one, the reverse is not true.

Those especially interested in scale analysis may ask whether scaling was used to study the independence of the three dimensions (recognition, evaluation, and equivalence). The notion that more than one variable is being measured if the items do not scale is applicable in this connection. But this rule has a serious limitation: if the items do not scale, then more than one variable is involved; but if the items scale, it does not follow that only one variable is involved.

For whatever suggestiveness they might have, we present the results of scaling the tests two at a time. The reproducibility coefficients dropped slightly below 90 per cent for each scalogram—an insignificant drop.<sup>10</sup> This evidence may be interpreted in

one of two ways: (a) only one dimension is involved rather than three, or (b) dimensions are separate and distinct but nevertheless scale together. An example may help to clarify the latter interpretation, which is the one we favor. Latin and Mathematics items may form a scale but no one would claim that therefore Mathematics is Latin; neither does the fact that both are perfectly correlated with a common factor make them identical.

#### ITEMS RANKED BY DIFFICULTY

To determine which responses on all three tests are necessary prerequisites to other responses, a rank order of all items by difficulty was worked out. Items were arranged in a single scalogram and cutting points determined for each item. The number of scores above the first cutting point was then used as the criterion of difficulty. When first cutting points coincided, second cutting points were employed to differentiate items by relative difficulty. The rank order of items established by this method is shown in Table 3.

TABLE 3. ITEMS RANKED BY DIFFICULTY\*

Rank Order	Test	Item	Scores above the First Cutting Point
1	Equivalence	1	60
2	Value	1	57
3	Recognition	1	51
4	Recognition	2	47
5	Value	2	46
6	Equivalence	7	46
7	Equivalence	2	37
8	Equivalence	3	34
9	Recognition	4	32
10	Recognition	5	32
11	Equivalence	4	28
12	Recognition	3	25
13	Value	3	22
14	Recognition	6	14
15	Recognition	7	14
16	Value	4	14
17	Equivalence	5	8
18	Recognition	8	4
19	Value	5	1
20	Equivalence	6	0

\* The content of these items is given in the previously published paper, see Figure 1, "The Schedule."

	Fairview	University
9		
$r_{12}$	.80	.69
$r_{13}$	.86	.79
$r_{23}$	.84	.77
10		
Reproducibility Coefficients		
Tests		Test
1-2	.88	.96
1-3	.88	.95
2-3	.89	.93

Items are considered equal in difficulty if the number of cases or scores above the first cutting point does not differ by more than five.<sup>11</sup> The scalogram method allows the interpretation that to pass a certain item a person must have passed all items ranked in terms of difficulty as less than, or equal to, that item. This interpretation can be made only if the items form a scale.<sup>12</sup> Our data can be interpreted in this way as the scalogram for all three tests combined had a reproducibility coefficient of .87.

Ranking by scalogram has distinct advantages over the chief method now in use for the study of conceptual development in children. This latter method determines what children at a given age know "on the average" about a certain topic such as time, number, death, or social class.<sup>13</sup> Conclusions consist of statements like "at a given age the child knows this, and at another age he also knows that." This procedure fails to indicate how many children passed a more difficult item while failing an easier one, although most children of given ages who pass a difficult item also passed the easier ones.

Ranking by scalogram also makes possible the addition of items which represent other aspects of the concept money. The relative difficulty of these items, as well as their relationships with all other items, can be determined. The outcome of such analysis

then can be used to make increasingly detailed statements about developmental stages and their prerequisites. This procedure is especially fruitful since in this way conditions necessary to any stage of development for "money"—or probably any concept—can be studied.<sup>14</sup>

Another difficulty with the customary method is that it reveals only in a rather vague way that one item is both more difficult than another and is learned later. This difficulty has been met in at least two ways: (a) a single concept was broken down into a few of its many component aspects and their relationships shown, and (b) the use of logical notation revealed the logical operations implicit in the children's responses to each item as well as clarifying the connections between these operations (as will be shown in the next section).

#### CONCEPT FORMATION AND LOGICAL REASONING

An argument popular with child psychologists is that "the child's reasoning processes at the age of six are [not] essentially different from his reasoning processes at the age of twelve or eighteen."<sup>15</sup> This argument involves the following points. First, children's logic is "essentially" the same as adults'. Second, the more frequent errors committed by the child are a con-

<sup>11</sup> This figure is arbitrary and merely reflects our judgment that small differences can be reasonably attributed to chance.

<sup>12</sup> Other methods for ordering items by difficulty do not establish whether less difficult items are prerequisite to more difficult ones. Contingency tables show whether in any pair of items one is more difficult than the other; the combined results of all such paired comparisons yield therefore a rank order of items by difficulty. Another index of difficulty is the relative number of persons giving the "best" answer to a question.

<sup>13</sup> L. B. Ames, "The Development of Sense of Time in the Young Child," *Journal of Genetic Psychology*, 68 (1946), 97-125; L. Long and L. Welch, "The Development of Abilities to Discriminate Numbers," *Journal of Genetic Psychology*, 59 (1941), 377-387; M. Nagy, "The Child's Theories Concerning Death," *Journal of Genetic Psychology*, 73 (1948), 2-37; C. B. Stendler, *Children of Brasstown*, 1949.

<sup>14</sup> If variables can be distinct but yet scale together—as we have previously suggested—then the situation may arise when an item unrelated to the concept being studied may appear as a "necessary condition" for a given level of development. Such an item is necessary only in the sense that if a child has reached a given level of development with respect to the concept studied, then he also invariably has attained a given level with respect to the irrelevant item. However, this is a shift in the meaning of this term "necessary condition." If such a situation arose then it would be necessary to determine empirically whether a given level could be reached with this item absent.

<sup>15</sup> A. Jersild, *Child Psychology*, 3rd edition, 1947, p. 380. See also: L. Thorpe, *Child Psychology and Development*, 1946, pp. 550-552; V. Hazlitt, "Children's Thinking," *British Journal of Psychology*, 20 (1929-30), 354-361; S. Isaacs, *Intellectual Growth In Children*, 1930, p. 57.

sequence of false premises rather than of inferior logic or inability to reason abstractly. Third, these false premises are the result of inadequate "experience" with the given subject matter. Fourth, adults—like children—when confronted by unfamiliar subject matter are likely to commit errors in logic and judgment. The view that children must have certain requisite experiences is an efficient explanation of why they cannot be forced before their time to master certain concepts.

This argument is fully as important for social psychologists as it is for child psychologists. For if, actually, children do not reason "essentially" the same as adults then a peculiarly subtle kind of anthropomorphizing of the child is being committed by many investigators. It is virtually a canon in social psychology that we must not attribute to children the perceptual and mental sophistication that is granted to most adults, nevertheless a considerable number of personality theorists attribute substantially the same sort of perception, motives, and rationales to children as to adults.

Our study, although confined to a limited topic, supports the opposite hypothesis that significant differences in logical reasoning among children of different ages exist. In Tests 2 and 3 certain items cannot be passed unless certain logical operations were previously mastered.<sup>16</sup> For purposes of brevity only certain items from Test 3 will be discussed although the conclusions cited apply equally to the remaining items and to all of Test 2.

The order of items in Test 3 by difficulty can be seen from Table 3. Table 4<sup>17</sup> gives excerpts from three items and the logical operations involved in passing each item. These three particular items were selected because they are, respectively, next to the easiest, the middle, and next to the most difficult items. (The easiest item, 1, involves mere sensorimotor matching of coins by color and size, not logical relation; while the

most difficult, 6, involves extremely intricate logical relations.)

The three items analyzed in Table 4 involve the logical operation of "logical addition" as well as the propositional relations of "equality" and "equivalence." These matters have been concisely described by Cohen and Nagel:<sup>18</sup>

Consider . . . the two classes "English books" and "French books." The class which contains *either French or English books* will be said to be the *logical sum* of these classes. The operation of combining them in this way will be called *logical addition*. If *a* and *b* are classes, their logical sum is represented by  $a+b$ . . . . The symbol  $+$  is employed because logical addition has certain formal analogies to the addition of ordinary arithmetic. . . . We may define the *equality* of two classes in terms of mutual inclusion. Class *a* is equal to *b* if *a* is included in *b* and *b* is included in *a*. . . . In symbols,  $(a=b) \equiv (a \text{ is included in } b) \cdot (b \text{ is included in } a)$ , where the sign  $\equiv$  indicates *equality between classes*, the sign  $\equiv$  indicates *equivalence between propositions*, and the dot ( $\cdot$ ) indicates the *joint assertion* of two propositions.

The chief points to be noted about Table 4 are as follows. First, the number of classes involved increases rather regularly as the items increase in difficulty. Second, the relationships between classes become correspondingly more numerous. Third, these relationships may be expressed as propositions holding between classes, hence the passing of each item involves the use of increasingly elaborate propositions about the classes of coins. Fourth, the passing of more difficult items is, for the most part, based upon the simpler logical knowledge involved in passing simpler items. Fifth, a very young child's knowledge of a given concept (for instance "dime") may be thought of as partial: (a) partial in terms of his knowledge of subject matter, and (b) also in the logical sense that certain propositions cannot be made about a class because the child has not yet learned the requisite logical operations which he needs to perform on the class and related classes.

<sup>16</sup> Test 1 involves no logical operations since it tests recognition.

<sup>17</sup> Only part of which is reproduced here because of its great length and complexity.

<sup>18</sup> M. Cohen and E. Nagel, *Logic and Scientific Method*, pp. 122-123.

The use of logical notation in analysis is a reminder that logical operations and relations are intrinsic to the classification of objects. It is obvious that concepts vary in degree of abstractness. This being so, it should be equally obvious that logical manipulations are involved explicitly or implicitly in concept-making, so that these

correspondingly vary in complexity as concepts vary.<sup>19</sup>

<sup>19</sup> Piaget in his later works has extensively and effectively utilized the notation of symbolic logic, with results very similar to ours. He has also discussed "pre-concepts" which are the child's notions that do not yet fulfill the requirements of concepts, i.e., general classes and relations between classes.

TABLE 4. LOGICAL ANALYSIS OF SELECTED EQUIVALENCE ITEMS

Item	Directions	Coins Used	Propositions Involved in Transaction*
2	(a) Give back the same worth.	D—10P	<ol style="list-style-type: none"> <li>1. A penny is equal to a penny.</li> <li>2. A dime is equal to a dime.</li> <li>3. A penny is less than a dime.</li> <li>4. Nine pennies or fewer are less than a dime.</li> </ol>
	(b) Give back the same worth.	D—5P, N	<p>All the above propositions and also:</p> <ol style="list-style-type: none"> <li>1. A nickel is equal to a nickel.</li> <li>2. A penny is less than a nickel; and a nickel is less than a dime.</li> <li>3. Five pennies equals a nickel.</li> <li>4. Four pennies or fewer are less than a nickel.</li> <li>5. Two nickels are equal to a dime.</li> </ol>
4	Which will buy the most?	P, 2N, 2D or Q, N, P	<p>All the above propositions and also:</p> <ol style="list-style-type: none"> <li>1. Ten pennies are equal to a dime.</li> <li>2. A quarter is equal to a quarter.</li> <li>3. Twenty-four pennies or fewer are less than a quarter.</li> <li>4. Twenty-five pennies are equal to a quarter.</li> <li>5. Five nickels equal a quarter.</li> <li>6. Four nickels or fewer are less than a quarter.</li> <li>7. Two dimes are less than a quarter.</li> <li>8. A penny is less than a nickel, a nickel is less than a dime, and a dime is less than a quarter.</li> </ol>
5	Match what I give you. You can use money from my other pile here if you wish.	Receives 5N and 4P. Gives Q, D. Takes 6 P.	<p>All the above propositions and also:</p> <ol style="list-style-type: none"> <li>1. Five nickels and four pennies are less than a quarter and five pennies.</li> <li>2. Five nickels and a penny are more than a quarter.</li> <li>3. The following steps represent a further partitioning of the concept "dime" and the point at which the penny becomes a genuine counting unit. <ol style="list-style-type: none"> <li>a. One penny and nine pennies equal a dime.</li> <li>b. Two pennies and eight pennies equal a dime.</li> <li>c. Three pennies and seven pennies equal a dime.</li> <li>d. Four pennies and six pennies equal a dime.</li> </ol> </li> </ol>

\* These propositions could be expressed more concisely in symbols though perhaps not more clearly. To an adult any two of these propositions may seem one and the same: for example, if four pennies are less than a nickel, then five pennies are equal to a nickel. But this assumes the child reasons precisely as adults do. The child may say four pennies are less than a nickel and believe that five pennies are less—or more!—than a nickel. Likewise he may know the correct series—penny, nickel, dime, quarter—but not know three nickels are worth less than a quarter.

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Our evidence suggests strongly that young children's logical reasoning is inferior to adults, and that the claims that children reason abstractly should be qualified with statements as to the degree of abstraction involved.<sup>20</sup> Our hypothesis, as well as the more traditional one, explains why children cannot be forced to learn certain subject matters and concepts "ahead of time." Further, when children do not have the concepts which are requisite to certain adult motives, rationales and roles, then these latter should not be attributed to the children. Social psychologists who ignore or understress the importance of concepts, i.e., language, in organizing human behavior are likely to be found anthropomorphizing the child.

#### SIGN SERIES AND BEHAVIORIAL ORGANIZATION

The data, viewed as a progression of increasingly complex logical relations, have implications also for the reductionism implicit in the thinking of many psychologists. Most animal experimentation in this country is based avowedly on the assumption that animal behavior, although less complex, is "essentially" the same as human behavior, and that both behaviors can be accounted

for by the same principles. Probably most social scientists do not agree with this view and their disagreement is expressed in the time-honored formula: animals have signals but only man has symbols.

It might be more profitable to view all behavior as organized around signs of varying complexity. This sign series, as it has been called,<sup>21</sup> would account for differences between the activity of lower, higher, and human animals without the necessity for sharply separating one kind of signs from another. The notion of sign series, however, does not in itself answer the question of whether or not each successive level of sign behavior can be reduced to lower levels.

Our Table 4 has some bearing on this controversy. The passing of each more difficult item requires increasingly complex logical manipulation. Although higher order propositions or relations may rest upon lower order ones, they are not thereby reducible. It would require considerable dialectical skill to claim that they are "really the same although more complicated."

This interpretation is not a negation of the reductionist position—for it cannot be empirically refuted—but represents rather a sharpening of the important problem of the sign series. We view the sign behavior of very young children who have learned to speak as intermediate between that of highest animals and that of adult humans. The research task set by our treatment is that of describing in detail these intermediate steps, and relating them to both simpler and more complex ones.

<sup>20</sup> Here is an example of how a child applies what he learned in a concrete situation to a situation which calls for a higher degree of abstraction and logical reasoning. The child has learned empirically in store situations that the more pieces of gum, the more pennies are needed. We show him one stick of gum and ask what it costs. "One penny." Then we break the stick in half and repeat the question. "Two cents." Then we tear one of the halves in half and again asked the question. "Three cents."

<sup>21</sup> C. Morris, *Signs, Language and Behavior*, 1946, esp. pp. 52-55; and A. Lindesmith and A. Strauss, *Social Psychology*, 1949, pp. 39-43.

## THE RELATIVE POSITION OF THE NEGRO MALE IN THE LABOR FORCE OF LARGE AMERICAN CITIES\*

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IT is the purpose of the present paper to measure degrees of certain types of Negro-white male *inequality* in ninety cities which in 1940 had populations of 100,000 or more,<sup>1</sup> and to attempt to ascertain some of the characteristics which differentiate those cities with the greatest inequality from those with least inequality. The two specific types of inequality to be measured are employment and occupational level. These indicate, respectively, degree of effective labor force participation, and for those employed, the level at which participation is attained. They are measured by ratios of per cent of Negroes employed to per cent of whites employed and of per cent of Negroes in semi-skilled and higher occupations to per cent of whites in similar brackets. In both cases, values of 1.00 indicate equality, values less than 1.00 indicate degrees of Negro disadvantage, and values above 1.00 indicate degrees of Negro advantage.

For purposes of computing the index of employment equality, employment rates are defined as the per cent of persons in the labor force who are employed.<sup>2</sup> In formu-

lating an index of occupational equality, occupations simply are dichotomized into unskilled-menial and semi-skilled and higher, according to the operational principle of placing those occupations with generally excess Negro membership in the lower half. This places farm and nonfarm labor, domestic and other service work (but excluding protective service) in the unskilled-menial bracket.<sup>3</sup>

Equivalent values for these indices computed for different cities may be regarded as equivalent in meaning since, in practice, there is a defined possible range from 0.00 to 1.00 in each instance, only one case being found of an index value exceeding 1.00, and because the base value does not constitute a limiting factor on possible index values. However, it is not possible to dismiss the objection that the categories used may not have comparable meanings for the Negro and white populations.

Five groups of factors, based on hypotheses frequently found in the race relations literature, were examined for their association with the two indices. Besides *region*, the groups of factors were as follows: (1) white and Negro employment and occupational levels; (2) labor force variables, including labor force and employment rates for females; (3) demographic variables, including city size, proportion of population Negro, and changes in the total and in the Negro population through migration; (4) industry composition, measured by per cent

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<sup>3</sup> Though classed as "service work," the protective service category has attributes probably closest to the skilled worker group, and shows one of the highest exclusion rates toward Negroes. In the urban labor force, farmers make up an inconsequential segment, and have been placed in the upper group of occupations with proprietors, in spite of higher Negro than white membership.

\* This study was begun as part of a University of Chicago Ph.D. thesis under a Social Science Research Council fellowship. A grant from the University of California Faculty Research Committee and the computational assistance of Mr. James Reeves, graduate student, enabled its completion.

<sup>1</sup> Two of the ninety-two such cities were omitted because the small number of Negroes in the labor force rendered percentages unreliable.

<sup>2</sup> The possible inadequacies of labor force delimitation in the 1940 Census, and specifically the possible distorting effect of these inadequacies on employment rates, must be recognized as affecting the validity of the index. Cf. Louis J. Ducoff and Margaret J. Hagood, *Labor Force Definition and Measurement*. New York: Social Science Research Council, 1947, Bulletin No. 57; and Ralph H. Turner, "The Nonwhite Male in the Labor Force,"

of employed males in each of eight major industries; and (5) "social organization" variables, of which housing and education level were used.

In keeping with the formulation of the major question of this paper, the ninety cities have been classified into four types as follows: type HH (for high-high), those above the median in both employment and occupational equality; type HL (for high-low), those above the median in employment equality but below the median in occupational equality; type LH, below the median in employment equality and above the median in occupational equality; and type LL, below the median in both. The differences in mean city size between groups HH and LH and between groups HL and LL can be taken as measuring association of employment equality with city size, holding occupational equality relatively constant. Similar mean differences between HH and HL and between LH and LL indicate association of occupational equality with the factor under consideration while holding employment equality constant. The four-fold typology also permits search for any significant associations with these two types of equality *taken together* which are not revealed by their examination as separate variables.

Mean differences between Southern and non-Southern cities have been computed wherever it appeared useful, and a separate study of the factors in relation to the sixty-seven non-Southern cities alone has been made. The mean of the top twenty-two non-Southern cities was compared with the mean for the bottom twenty-two, for each index separately. This will be referred to for convenience throughout the paper as the "high-low analysis." To take account of variations over the entire range, correlation ratios were also computed and the column means plotted for non-Southern cities only.

Characteristics of the indices themselves and of their interrelations are summarized in Table 1. Except that the upper range of the occupational index is misleading, since the second highest value is .742, or about .2 lower, all the measures show the

TABLE 1. CHARACTERISTICS OF THE INDICES

Characteristic	Employment Index	Occupation Index
Mean	.813	.428
Standard Deviation	.0955	.1007
Range:		
Upper	1.022	.949
Lower	.575	.223
Region Mean*		
South	.892	.397
North and West	.786	.439

\* The regional difference in mean employment indices is significant at the .001 level. The difference in the mean occupational indices is not significant.

distribution to run much higher in the case of employment equality than of occupational equality.

The various measures reveal slight negative associations between occupational equality and employment equality, which are not, however, statistically significant.<sup>4</sup> The association with regional differences is highly significant for employment equality but not for occupation. Forty-two and 54 per cent respectively of the cities in types HH and HL are Southern cities, as compared with none and 4 per cent of the LL and LH groups.

Both of the indices have been tested for association with the white employment rate and proportion of employed workers in semi-skilled and higher occupations. (Tables 2 and 3.) The employment index is not significantly associated with either variable by any of the three tests used. In combination with the highly significant correlations between white and Negro employment rates ( $r=.52$  for males,  $r=.37$  for females, for

<sup>4</sup> Above .05 is taken as the level of rejection. Where  $P$  is .05, reference to "moderate significance," will be made. An unqualified reference to significance will always mean a  $P$  value of .01 or less. Significance of mean differences has been tested by the "t" distribution, using formulae from Margaret J. Hagood, *Statistics for Sociologists*, New York: Reynal and Hitchcock, 1941, Chap. 17. Significance of the correlation ratio has been tested by formulae from Charles C. Peters and Walter R. Van Voorhis, *Statistical Procedures and Their Mathematical Bases*, New York: McGraw-Hill, 1940, Chap. XI.

ninety cities), the evidence suggests that Negro employment *level* is importantly a function of the general community employment level, but that the interracial employment *differential* is not such a function.

Of more limited scope is the significant association of high occupational level with relative employment equality *in those cities with relative occupational equality* but not in cities with relative inequality. Stated as a broader generalization, the observation might take the following two-fold form: (a) the *degree of relationship* between a city's occupational level and inter-group equality of employment increases with the degree of equality of occupation; (b) this relationship is a positive association between the city occupational level and employment equality. This relationship is not explained by regional difference, since approximately equal

proportions of Southern cities are found in both the HH and HL groups.

Within the North and West only there is a significant *negative* association by the high-low test between Negro male employment and occupational equality, with a significant negative association between occupational equality and white employment by the high-low and correlation ratio tests. There appears, then, to be some incompatibility between high employment rates and inter-group occupational equality, which the graph of column means shows to be curvilinear, quite steep at low employment levels and nearly level at the upper rates.

Some of the factors examined show no significant relationships with the indices, and may be disposed of immediately. These include Negro male labor force rate, non-white net migration (1935-1940) as per

TABLE 2. ASSOCIATION OF SELECTED VARIABLES WITH EQUALITY INDICES BY "FOUR-TYPE" ANALYSIS AND WITH REGION<sup>a</sup>

Variable	Direction and Significance Level of the Difference Between Type Means <sup>b</sup>									
	Employment Equality				Occupational Equality				South Minus Non-South	
	HH-LH		HL-LL		HH-HL		LH-LL			
	Direction	Significance	Direction	Significance	Direction	Significance	Direction	Significance	Direction	Significance
Negro male employment	+	.001	+	.001		NS		NS	+	.001
Negro female employment	+	.001	+	.001		NS		NS		
White female employment		NS		NS		NS	-	.05		
White males in unskilled occupations	-	.01		NS		NS		NS	-	.001
Negro female labor force	+	.01	+	.01		NS		NS		
Per cent of population Negro	+	.05	+	.05		NS		NS	+	.001
In-migration		NS		NS	-	.05	-	.05		NS
Per cent of employed males in:										
Construction	+	.01	+	.001		NS		NS	+	.001
Manufacturing	-	.01	-	.01		NS		NS	-	.001
Trade	+	.01	+	.02	-	.01	-	.02	+	.001
Personal Service	+	.001	+	.02		NS		NS	+	.001
Median schooling:										
Negro males		NS		NS		NS	-	.01	-	.001
All persons over 24		NS		NS	-	.05	-	.01		NS

<sup>a</sup> All data are from the various reports of the 1940 U. S. Census. Only variables yielding some significant relationship are reported in the table.

<sup>b</sup> The table should be read as follows: The mean Negro male employment rate is higher in group HH than in group LH (HH minus LH yields a positive value), and the difference is significant by the "t" test at the .001 level; etc.

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cent of 1940 nonwhite population, total population of the city, and per cent of residences needing major repairs.

Considering labor force variables, employment equality is associated with high Negro female labor force rate, with low white male labor force rate, and with high Negro female employment rate. The last is explained through the high association between male and female Negro employment rates, and the former is consistent with the considerable correlation between employment and labor force rate for Negro women. Occupational equality is associated at bor-

derline significance with high white male and female labor force rates, and may be negatively associated with white female employment as it was with white male employment.

Preliminary findings of moderate positive association between proportion of the population who are Negro and employment equality are a function of the much higher proportion of the population of Southern cities who are Negro. When only the North and West are considered there is a significant *negative* association between size of the Negro population and employment

TABLE 3. ASSOCIATION OF SELECTED VARIABLES WITH EQUALITY INDICES IN NON-SOUTHERN CITIES ONLY BY "HIGH-LOW" TEST AND CORRELATION RATIO\*

Variable	Direction and Significance Level of the Difference							
	Employment Equality				Occupational Equality			
	High Minus Low		Correlation Ratio		High Minus Low		Correlation Ratio	
	Direction	Significance	Coefficient	Significance	Direction	Significance	Coefficient	Significance
Negro male employment	+	.001	.90	.01	—	.01	NS	
White male employment		NS		NS	—	.05	— .47	.01
Negro female employment	+	.001	.52	.01		NS	NS	
White female employment		NS		NS	—	.01	NS	
White male labor force	—	.02	— .505	.01	+	.05	NS	
White female labor force		NS		NS		NS	.69	.01
Negro female labor force	+	.02	.34	.05		NS	NS	
Per cent of population Negro	—	.01	— .42	.05	+	.01	NS	
Net migration 1935-40	+	.02	.37	.05		NS	NS	
In-migration 1935-40		NS		NS	—	.001	— .59	.01
Nonwhite in-migration 35-40	+	.05	.51	.01	—	.001	— .44	.05
Per cent of employed males in:								
Construction	+	.02		NS	—	.001	— .51	.01
Manufacturing	—	.001	— .48	.01	+	.001	.47	.01
Transportation		NS	.44	.05	—	.01	— .57	.01
Trade	+	.001	.45	.01	—	.001	— .38	.05
Finance	+	.01		NS	—	.01	— .41	.05
Personal Service	+	.01	.38	.05		NS	NS	
Professional	+	.01	.45	.01		NS	.54	.01
Government	+	.01		NS	—	.02	NS	
Median schooling:								
Negro males	+	.001	.51	.01	—	.001	— .42	.05
All persons over 24	+	.02		NS	—	.001	— .57	.01

\* The table should be read as follows: The mean Negro male employment rate is higher for the twenty-two cities highest in employment equality than for the twenty-two lowest (high employment index group minus low employment index group yields a positive value), and the difference is significant by the "t" test at the .001 level; the correlation ratio between Negro male employment and employment equality is .90, and is significant at the .01 level; etc.

equality, but a positive association with occupational equality. A regional pattern in this instance appears to reverse and to minimize relationships: the regional difference—high employment equality associated with large Negro population—is the reverse of the relationship with region held constant.

Interpretation in terms of Negroes moving in large numbers into communities with relative job equality, and relatively high Negro unemployment resulting therefrom, is vitiated by an absence of association with net nonwhite migration in the preceding five years, and the associations in the reverse direction when only in-migration is considered.<sup>5</sup> The analyses for the North and West only reveal that Negroes have not migrated to those communities with relative occupational equality to the degree that they have to other communities, but that they have migrated in larger numbers to those communities with relative employment equality. Taken in conjunction with the net migration findings, it may be proposed that the cities with greatest occupational equality are those with the least mobility of the Negro population, while greatest employment equality goes with high mobility of the Negro population.

Since the figures for *total* in-migration and net migration, white plus nonwhite, reveal the same associations with respect to occupational equality, it appears that the less mobile cities are characterized by the greatest inter-group equality of occupation, and that mobility of the *Negro* population may not be a factor except as it is part of the total mobility of the community. The association of employment equality with in-migration (which is significant for Negroes but not for the total population) indicates, however, that the mobility of the Negro population, as distinguished from population growth through migration, is associated with relative equality of employment. The association with total net migra-

tion indicates somewhat more employment equality for those cities which had lost least in population during the preceding half decade. In the case of each of these demographic variables, the generalizations have applied only when the South was omitted from consideration.

Some significant association is revealed between the per cent of male employed workers who are in each of the eight selected industries and either or both of the indices; and, in each instance where there is a significant association with both, the direction of association is always *opposite* for the two indices. Intercorrelations between four of the most important of these variables (Table 4) indicate that probably fundamentally only two types of cities, manufacturing and non-manufacturing or trade-and-financial cities, are involved. Manufacturing cities are marked by relative inter-group equality of occupation but marked inequality of employment; trade and financial cities are marked by gross inequality of occupation but relative equality in employment rates.

Since differences in community occupational level have already been ruled out as explanatory of these relationships, the difference, then, must lie in the Negro occupational level, possibly in availability of semi-skilled and some skilled work, or in a smaller proportion in service work in manufacturing towns. As a test of this latter hypothesis, the nineteen non-Southern cities which rank highest in both employment equality and per cent of the employed male population in manufacturing were compared with the eighteen ranking lowest in both, with respect to per cent of the employed Negro male population in service work (other than domestic and protective) and per cent in the operatives bracket. The differences (Table 5) are the most significant secured in this study, and show more than twice as large a proportion in the operative category and less than half as large a proportion in service work in the manufacturing-occupation equality cities.

The employment equality difference may be a function of the manufacturing city's

<sup>5</sup> By considering both net migration and in-migration it has been possible to assess the importance of degree of population mobility in and out of a community apart from population growth or loss through migration.

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TABLE 4. INTERCORRELATIONS BETWEEN PERCENTAGES OF THE MALE EMPLOYED POPULATION WORKING IN FOUR SELECTED INDUSTRIES, IN NINETY CITIES

Industry	Construction	Manufacturing	Trade
Manufacturing	-.716		
Trade	.627	-.809	
Personal Service	.819	-.682	.705

lesser protection afforded against discriminatory employment in the unsegregated occupations, practices deriving from the large and conspicuous Negro population, or other factors, and may be more indicative of overt inter-racial friction than the occupational index.

Finally, median years of school completed for both the Negro males alone and the total population is associated positively with employment equality and negatively with occupational equality. These associations are probably adequately accounted for already by the city's industrial composition.

The preceding survey of several sorts of variables points toward the industrial character of the city as most important in relation to the Negro's relative economic position

TABLE 5. MEAN PER CENT OF EMPLOYED NEGRO MALES IN OPERATIVE AND SERVICE WORK FOR SELECTED NON-SOUTHERN CITIES ON THE BASIS OF MANUFACTURING AND OCCUPATIONAL EQUALITY

	Service Work (other than Domestic and Protective)	Operatives
Mean for cities with highest per cent in manufacturing and highest occupational index value (N=19)	22.79	25.65
Mean for cities with lowest per cent in manufacturing and lowest occupational index value (n=18)	52.46	10.96
Probability of observing such a difference arising from chance	.001	.001

and as explaining associations with the labor force, education and some demographic variables. It renders doubtful attempts to generalize about equality as such, since the two different sorts tested here have in many respects complementary relations. And it offers an explanation for certain facts of inequality in terms of the existence of types of occupation and industrial conditions which provide an opportunity for the Negro, thus suggesting what may be more profitable ways of explaining differential patterns in race relations than the search for differences in prejudice and discrimination. Some light is also cast on regional difference by noting that the higher index of employment equality and slightly lower index of occupational equality reported for Southern cities is consistent with what would be expected from the regional differences in predominance of certain industries, in net migration and in white male employment, but is inconsistent with what would be expected on the basis of relative size of the Negro population and level of Negro education.

The findings themselves have underlined the oversimplification of the occupational dichotomy used in the index, and have pointed out the need for a far more detailed study taking into account the interrelationship of detailed industries and occupations and Negro participation in them, and effectively holding constant the influence of industry to ascertain what other variables may operate independently of these. Some of the findings are subject to interpretation under the alternate hypothesis that they reflect variations in the relation of the city limits—on which these statistics are based—to the "natural community," and subsequent study using the latter units is desirable. No attempt has been made as yet to examine the cities which constitute exceptions to the findings so as to effect modifications of the generalizations. And the use of some typologies of American cities, as distinct from the factor approach, is worth exploring. Finally, with more cities and more variables, a factor analysis to deal with the extensive interrelationship of the variables might well be attempted.

# NOTES ON RESEARCH AND TEACHING



## THE LABOR FORCE AS A FIELD OF INTEREST FOR THE SOCIOLOGIST

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By reason of our historical but artificial division of labor, the labor force as a field of research and teaching has been almost exclusively the province of the economist,<sup>1</sup> and more recently of the psychologist. The sociologist, however, has increasingly become interested in the various aspects of the labor force; and in focusing on the labor force, the contemporary sociologist is with considerable delay following the lead of some of the pioneers of sociology—as for example, Spencer and Durkheim.<sup>2</sup>

The "labor force" in current Census and statistical usage consists of all persons "working" or "seeking work" in accordance with a definite conceptual framework and as determined by prescribed methods of measurement and standard census operating procedures. The labor force consists, then, of that part of the total population engaged in, or seeking, "work"—an activity defined in our culture as one contributing to the production of goods or services in exchange for monetary remuneration or its equivalent. As a field for sociological research, the labor force may be narrowly conceived as being restricted to the size, composition and dynamics of "workers" in the population. Or it may be broadly conceived as including also investigation of the various factors, including the cultural and social psychological, which are related to labor force participation; of the various fields of interaction

within the labor force including both formal and informal relations among various categories of workers, most prominent of which, perhaps, is that between "labor" and "management"; and of relations between workers and non-workers. The labor force can be conceived both as a dependent and independent variable in relation to other aspects, cultural and personal, of society. It is in this broader sense that this essay treats the field as an area of research interest to the sociologist.

There are a number of aspects of the labor force which merit the special attention of the sociologist: first, because of the significant way in which labor force data, broadly conceived, can illuminate other cultural, institutional, and personal phenomena; and second, because of the contribution which the sociologist can make to a better understanding of labor force structure, processes and problems. Some of the more important fields of sociology in which attention to the labor force is particularly called for, and some of the implications of labor force analysis for sociological research are discussed in the materials which follow.

### *Demography*

Sociology has for some years been the major discipline concerned with the field of population. Formal demography, as evidenced by the earlier population literature, was almost exclusively concerned with the size, growth, and components of growth of population—fertility, mortality, and migration; and with selected attributes of the population, particularly age and sex.

Contemporary students of population, however, are directing increasing attention to the labor force, or at least to its size, structure and dynamics, particularly as related to total population and its structure and dynamics.<sup>3</sup>

<sup>3</sup> John Durand, *The Labor Force in the U.S., 1890-1960*, New York: Social Science Research Council, 1948.

A. J. Jaffe, and Seymour L. Wolfbein, "Demographic Factors in Labor Force Growth," *American Sociological Review*, II (August 1946), 392-396.

<sup>1</sup> This essay was prepared at the invitation of an economist, Seymour L. Wolfbein, who served as Chairman of the session on the Labor Force at the annual meeting of the American Sociological Society held in Denver, September 7 to 9, 1950.

<sup>2</sup> See Herbert Spencer, *Principles of Sociology*, Vol. II, Part III, New York: D. Appleton & Co., 1897.

George Simpson, *Durkheim on the Division of Labor in Society*, New York: The Macmillan Co., 1933.



Also of interest to the population student and sociologist have been the problems of concept and measurement involved in the quantification of labor force data.<sup>4</sup> The close interrelationship between population and labor force size, composition and change obviously call for continuing investigation and analysis and are likely to receive them. Similarly the problems of conceptual framework and measurement are likely to continue to receive close attention, especially by government, labor and business statisticians and economists, as well as sociologists.

Less obvious and calling for more intensive, and qualitative as well as quantitative, investigations are the factors—cultural, institutional and personal, as well as economic—which account for the differences between the structure and dynamics of the total population and of the labor force. For example, the decreasing labor force participation rates of youth and older persons in the United States is only to a minor, if any, extent, a function of the changing age structure of the total population. The trend towards declining proportions of younger and older persons in the labor force is to be accounted for by cultural and institutional changes and by changes in the attitudes and values systems of our people.<sup>5</sup>

Gladys L. Palmer, *Labor Force Dynamics and Economic Change in a Metropolitan Community*, Industrial Research Dept., Wharton School of Finance and Commerce, University of Pennsylvania (forthcoming volume).

<sup>4</sup>Louis J. Ducoff, and Margaret J. Hagood, *Labor Force Definition and Measurement*. New York: Social Science Research Council, 1947.

P. M. Hauser, "The Labor Force and Gainful Workers—Concept, Measurement and Comparability," *American Journal of Sociology*, 54 (January 1949), 338–355.

A. J. Jaffe, "The Application of Attitude Research Methodology Toward the Problem of Measuring the Size of the Labor Force," *International Journal of Opinion and Attitude Research*, I (December 1947), 45–54.

<sup>5</sup>See forthcoming report of the recent National Conference on Aging held under the aegis of the Federal Security Agency in Washington, D.C., August 12–15, 1950.

For problems relating to youth in the labor force see: John Durand, *op. cit.*, pp. 28 ff.

For problems relating to women see: *ibid.*, pp. 22 ff.

Hazel Kyrk, "Who Works and Why?," *Annals of the American Academy of Political and Social Science*, May, 1947.

Frieda S. Miller, "Women in the Labor Force," *Annals of the American Academy of Political and Social Science*, May, 1947.

Women, the young, and the old, to a considerable extent make up the "marginal" elements of the labor force—marginal in the sense that the proportion of these categories of the population who enter and leave the labor force during the course of a year or the course of a business cycle—quite apart from secular trend—fluctuates greatly. Research into the factors which account for the high mobility rate of these groups into and out of the labor force would do much to bolster our understanding of labor force activity as well as to illuminate the cultural, institutional and personal effects of such mobility.

Other fertile fields of investigation of labor force phenomena by the sociologist are afforded through the application of demographic interests and techniques to labor force data. For example, the collection and analysis of data on labor force migration, international and internal, is an almost indispensable aspect of studying population migration in general. The coordinated study of the migration of total population and the labor force would undoubtedly do much to provide a better understanding of the factors in both in- and out-migration.<sup>6</sup>

The application of methods of life table construction and analysis to labor force phenomena promises to open up new and interesting avenues

<sup>6</sup>Dorothy Thomas, *Research Memorandum on Migration Differentials*, New York: Social Science Research Council, 1938, pp. 126 ff.

Carter Goodrich, *Migration and Planes of Living*, Philadelphia: University of Pennsylvania Press, 1935, pp. 10 ff.

C. W. Thornthwaite *Internal Migration in the U. S.*, Philadelphia: University of Pennsylvania Press, 1934.

Dudley Kirk, *Europe's Population in the Inter-war Years*, Princeton: League of Nations, pp. 97 ff.

Eugene M. Kulischer, *Europe on the Move*, New York: Columbia University Press, 1948.

Clark Kerr, "Migration to the Seattle Labor Market Area, 1940–1942," *University of Washington Publications in the Social Sciences*, 2 (August 1942), 129–188.

Department of Commerce, Bureau of the Census, *Current Population Reports*, "Employment Characteristics of Migrants in the U. S., April 1948," Series P-50, No. 10, Washington, D. C., and other publications. See: *List of Publications Issued*.

Donald J. Bogue, "An Exploratory Study of Migration and Labor Mobility, Using Social Security Data," *Scripps Foundation Studies in Population Distribution*, No. 1, June, 1950.

See also publications of the International Labor Organization.

of investigation.<sup>7</sup> Not unrelated to the subject of applying life table methods to labor force analysis is the investigation of differential occupational mortality.<sup>8</sup> This is an area of research badly restricted in many parts of the world, including the United States, by the lack of adequate data—data which in the United States can be made available only through the combined resources of the Bureau of the Census and the National Office of Vital Statistics. Such data, among other things, may in time also permit the construction of exceedingly interesting occupational life tables.

Finally, reference should be made to the importance of using, and the opportunity in the 1950 census data to utilize, an important labor force datum, income, as a control for demographic as well as other forms of sociological research.<sup>9</sup>

#### HUMAN ECOLOGY

Human ecology as a field of research in sociology has been broadly described as a point of view and a method. Although formal definitions of this field of sociological research differ, such investigations as have been conducted have been primarily concerned with the spatial and distributive aspects of society.<sup>10</sup> It is desirable, however, that research in human ecology should also focus on the functional sub-structures of human societies as affected by the division of labor, technological development and general economic organization. Such research activity, among other things, would be concerned with the "sustenance" relations of the population<sup>11</sup> and with the basic economic structure of communities insofar as they serve as a foundation

for the cultural and institutional super-structure.

Although at least one of the founders of human ecology as a field in sociology has stressed the importance of investigating the economic and functional aspects of the community,<sup>12</sup> little has been done in this respect as yet in the way of empirical research. Aspects of the problem have been attacked by economists,<sup>13</sup> but rarely by sociologists in the context of a sociological research project.

One of the most fruitful ways of getting at the sustenance and functional economic relations within a community and between communities lies in the analysis of the labor force of the community classified by industrial and occupational composition. Such data are available from the decennial population census or can be obtained at relatively low cost from the Bureau of the Census through special tabulations.<sup>14</sup> Yet despite this relatively readily available source of data, the sociologist has done little with it. It is, in fact, an indication of the undeveloped character of the field of human ecology that relatively little attention is devoted to the labor force and its composition in either of the first two sociological textbooks in human ecology which have just appeared.<sup>15</sup>

Analysis of the differences in the occupational and industrial composition of the labor force of various areas within a metropolitan community would certainly illuminate an important aspect of community structure and interrelationships.<sup>16</sup> Such data based on the population census show

<sup>7</sup> Seymour L. Wolfbein, "The Length of Working Life," *Population Studies*, 3 (December, 1949), 286-294.

U. S. Department of Labor, Bureau of Labor Statistics, *Tables of Working Life*, Washington, D. C.: Government Printing Office, 1950, Bulletin No. 1001.

<sup>8</sup> P. K. Whelpton, *Needed Population Research*, Lancaster: Pennsylvania Science Press Printing Co., 1938, pp. 28 ff.

<sup>9</sup> See also section below on "Human Ecology."

<sup>10</sup> Louis Wirth, "Human Ecology," *American Journal of Sociology*, 50 (May 1945), 483-488.

J. A. Quinn, "Topical Summary of Current Literature on Human Ecology," *American Journal of Sociology*, 46 (September, 1940), 191-226.

<sup>11</sup> R. D. McKenzie, "Demography, Human Geography and Human Ecology," in L. L. Bernard, ed., *The Fields and Methods of Sociology*, New York: Long and Smith, 1934, pp. 58 ff.

<sup>12</sup> R. D. McKenzie, "Ecological Approach to the Study of the Human Community," in Park & Burgess, *The City*, Chicago: University of Chicago Press, 1925, pp. 63-79.

<sup>13</sup> D. B. Creamer, *Is Industry Decentralizing?*, Philadelphia, 1935.

Glenn E. McLaughlin, *Growth of American Manufacturing Areas*, Philadelphia, 1938.

National Resources Committee, *The Structure of the American Economy*, Part 1, Washington, D. C.: Government Printing Office, 1939, pp. 33 ff.

E. M. Hoover, *The Location of Economic Activity*, New York: McGraw-Hill, 1948.

<sup>14</sup> For example, in the 1940 Census see Vol. III, *The Labor Force*, and *Population and Housing—Census Tract Statistics*. Similar data will be available from the 1950 Census.

<sup>15</sup> Amos H. Hawley, *Human Ecology*, New York: The Ronald Press, 1950.

James A. Quinn, *Human Ecology*, New York: Prentice-Hall, 1950.

<sup>16</sup> See, for example, Calvin Schmid, "The Ecology of the American City," *American Sociological Review*, 15 (April 1950), 264-281.

place of residence of various occupational and industrial groups.

It is to be hoped that the 1950 Census data will provide both occupational and industrial labor force statistics for both the central cities and the surrounding areas of the new standard metropolitan areas.<sup>17</sup> Such tabulations will greatly increase the usefulness of the labor force data for ecological research. Moreover, the increase in the number of census-tract cities<sup>18</sup> and the increase in the number of tract grids for outlying as well as the central portions of metropolitan areas will greatly enrich research opportunity.

If occupational or industrial data by place of residence can be supplemented by comparable data showing place of work, important new knowledge will be gained on the way in which differences in place of work and place of residence help to structure our metropolitan areas and affect social and institutional organization.<sup>19</sup> Although the resources available to the Bureau of the Census did not permit the collection of data on place of work of the population as well as place of residence, some exploratory studies at least, are possible on the relation of place of residence to place of work.<sup>20</sup>

<sup>17</sup> The Census will use a newly defined standard metropolitan area for the 1950 Population Census and other censuses made up of one or more contiguous counties. See Executive Office of the President, Bureau of the Budget, "Standard Metropolitan Area Definition," January, 1949. (mimeographed)

<sup>18</sup> See forthcoming census tract publications of the Bureau of the Census for 1950; also Department of Commerce, Bureau of the Census, "Census Tract Areas, 1950," December 15, 1949.

<sup>19</sup> Kate K. Liepman, *The Journey to Work: Its Significance for Industrial and Community Life*, London: Kegan, Paul, Trench, Tribner & Co., 1944.

<sup>20</sup> In some areas state or local employment service statistics may provide data on place of work by industry reasonably comparable with the population data on place of residence by industry, for at least some industrial groups. This would permit the analysis by local areas within the metropolitan area on the relation between place of work and place of residence and thus provide some approximate answers. Another type of research is afforded by the analysis of residences of employees of large industrial establishments. A study of changes in residential location of long service employees and differences in location among various classes of employees may help to throw light on this general problem. Such exploratory studies are now being conducted by graduate students at the

Richer data are available for the analysis of the functional and sustenance relations between metropolitan areas and between the regions of the United States than for analysis within any given metropolitan area. Research opportunity for such inter-metropolitan area or inter-regional analysis will be greatly enriched if the 1950 Census plans are carried through. Of particular value will be the proposed tabulations for economic areas. The Bureau of the Census with the cooperation of other agencies has divided the United States into approximately 450 economic areas comprising the new standard metropolitan areas and homogeneous groupings of other counties.<sup>21</sup> Analysis of labor force data, particularly the occupational and industrial composition of the labor force by these new economic areas, and the broader regions into which they can be combined, may provide rich research opportunities for extending empirical ecological research into regional and national analysis.

Another extremely important item in the labor force data to be provided for the first time by the 1950 Census should also greatly enrich research opportunity for the human ecologist. This is the item of individual and family total income which will make available to the research sociologist the best measure of economic level which has yet emanated from the census. The income data will likely be available by census tracts for census-tract areas, for the new standard metropolitan areas, for the new urbanized areas,<sup>22</sup> and for the new economic areas. The availability of this direct measure of level of living, together with the new area units, permitting a great expansion in the analysis of data for "natural" as distinguished from political areas should open new vistas of analytical opportunity.

#### THE FAMILY

The study of labor force participation on a family basis would do much to illuminate many aspects of the family as a unit of social organ-

University of Chicago, the former by Leonard Breen, the latter by Helene Conant.

<sup>21</sup> For a description of these areas see Donald J. Bogue, "Economic Areas as a Tool for Research and Planning," *American Sociological Review*, 15 (June 1950), 409-416, and Bureau of the Census and Bureau of Agricultural Economics, "State Economic Areas of the U. S." Series Census-BAE, No. 15.

<sup>22</sup> See Department of Commerce, Bureau of the Census, "Urbanized Areas," November 15, 1949.



ization central to the interest of the sociologist, and also throw light on many puzzling aspects of labor force structure and dynamics.

Various students—mainly economists—have been interested in the problem of changing family labor-force participation and changing volumes of employment and unemployment within families as related to earnings of the primary worker in the family or to swings in the business cycle.<sup>23</sup> But much remains to be done before a clear understanding is gained of the way in which the family as a unit plays an important part in determining labor force participation under varying social and economic conditions. Analytical treatment of already available census materials<sup>24</sup> together with current census data and the returns of the 1950 Census will provide one avenue of research opportunity in this respect. More intensive sampling surveys and case studies, however, will come closer to getting at the various factors—cultural, institutional, social psychological and personal—which are determinants in varying labor force participation rates and their inter-relationships.

<sup>23</sup> Paul Douglas, *Wages and the Family*, Chicago: University of Chicago Press, 1925; *The Theory of Wages*, New York, 1934; Paul Douglas and Enka Schoenberg, "Studies in the Supply Curve of Labor: The Relation in 1929 Between Average Earnings in American Cities and the Proportions Seeking Employment," *Journal of Political Economy*, 45 (February 1937), 45-79.

Don D. Humphrey, *Family Unemployment*, Works Progress Administration, Washington, D. C.: Government Printing Office, 1940; "Alleged 'Additional Workers' in the Measurement of Unemployment," *Journal of Political Economy*, 48 (June 1940), 412-419.

W. S. Woytinsky, *Three Aspects of Labor Dynamics, Part III*, Washington, D. C.: Social Science Research Council, 1942, pp. 105 ff.

Clarence Long, *The Size of the Labor Force Under Changing Incomes and Employment*, Conference on Research in Income and Wealth, November, 1946 (mimeographed) 76 pp.; *The Labor Force in Wartime America*, New York: National Bureau of Economic Research, March 1944; "The Labor Force and Economic Changes," in Richard A. Lester, and J. Shuster, *Insights into Labor Issues*, New York: Macmillan, 1948, pp. 329-355. *Labor Force, Income and Employment*, National Bureau of Economic Research (forthcoming volume).

<sup>24</sup> See 1940 Population Census Reports: *Families & Employment Status; Family Wage or Salary Income, 1939*; also *The Labor Force: Employment & Family Characteristics of Women*.

Research such as that described above would throw considerable light on as yet largely unknown phases of labor force changes. Analysis of labor force participation rates, including both employment and unemployment rates and labor force entrance and egress on a family basis, should also help to provide a better understanding of the family as a changing social institution and as a group of interacting personalities. Fertile suggestions and patterns for research endeavor have in some respects already been outlined.<sup>25</sup> Many of the important areas of sociological research into the family summarized by Burgess and by Cottrell can be explored with labor force participation of family members considered as both a dependent and independent variable in relation to other aspects of family structure and process.<sup>26</sup> The opportunities for research in this matter are in the main still unexploited.

Finally, the availability in the 1950 Census of data on family income should provide an important opportunity for getting at differences in family organization and structure as affected by and as influencing income as well as labor force participation.

#### SOCIAL STRATIFICATION AND SOCIAL MOBILITY

The study of social stratification, at least in our society, necessarily involves consideration of labor force participation and status. The relative ranking of a person on an economic, honorific, political, or other scale, by whatever criteria, is almost certainly affected by the nature of his employment and its rewards—monetary, psychic, or other. Contemporary the-

<sup>25</sup> See footnote 23, also S. A. Stouffer, and P. Lazarsfeld, *Research Memorandum on the Family in the Depression*, New York: Social Science Research Council, 1937.

Robert C. Angell, *The Family Encounters the Depression*, New York: Scribner & Sons, 1936.

R. A. Cavan, and K. H. Ranck, *The Family and the Depression*, Chicago: University of Chicago Press, 1938.

Mirra Komarovsky, *The Unemployed Man and His Family*, New York: Dryden Press, 1940.

Paul Glick, "The Family Cycle," *American Sociological Review*, 12 (April 1947), 164-174.

<sup>26</sup> E. W. Burgess, "The Family and Sociological Research," *Social Forces*, 26 (October 1947), 1-6.

Leonard S. Cottrell, Jr., "The Present Status and Future Orientation of Research in the Family," *American Sociological Review*, 13 (April 1948), 123-129. See also discussion of this paper by E. W. Burgess, R. S. Cavan, & M. Komarovsky, pp. 129-136.

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orists of social stratification have explicitly or implicitly recognized the central importance of the person's role in the labor force as a factor in his ranking in the economic, social or political hierarchy. Yet despite the contribution of the theorists<sup>27</sup> in providing frameworks

<sup>27</sup> See, for example, Max Weber in H. H. Gerth, and C. Wright Mills, *From Max Weber's Essays in Sociology*, New York: Oxford University Press, 1945, Chap. 7.

T. H. Marshall, "Social Class," *American Sociological Review*, 9 (January 1934).

Hans Speier, "Social Stratification in the Urban Community," *American Sociological Review*, 1 (April 1936), 193-202.

Herbert Goldhamer, and Edward A. Shils, "Types of Power and Status," *American Journal of Sociology*, 45 (September 1939), 171-182.

Talcott Parsons, "An Analytical Approach to the Theory of Social Stratification," *American Journal of Sociology*, 45 (May 1940), 841-862.

Kingsley Davis, "A Conceptual Analysis of Stratification," *American Sociological Review*, 7 (June 1942), 309-321.

Emile Benoit-Smullyan, "Status, Status Types, and Status Interrelations," *American Sociological Review*, 9 (April 1944), 151-161.

For a frame of reference and elaborate research program centering largely around aspects of social stratification in which attention has been devoted to labor force status, see the works of W. Lloyd Warner & Associates listed below:

W. Lloyd Warner, and Paul S. Lunt, *The Social Life of a Modern Community*, Yankee City Series, Vol. 1, New Haven: Yale University Press, 1941.

W. Lloyd Warner, and Paul S. Lunt, *The Status Systems of a Modern Community*, Yankee City Series, Vol. II, New Haven: Yale University Press, 1942.

W. Lloyd Warner, and Leo Srole, *The Social Systems of American Ethnic Groups*, Yankee City Series, Vol. III, New Haven: Yale University Press, 1945.

W. Lloyd Warner, and J. O. Low, *The Social System of a Modern Factory*, Yankee City Series, Vol. IV, New Haven: Yale University Press, 1947.

W. Lloyd Warner, Marchia Meeker, and Kenneth Wells, *Social Class in America*, Chicago: Science Research Associates, 1949.

W. Lloyd Warner, and Associates, *Democracy in Jonesville*, New York: Harper & Bros. 1949.

The work of this group has been severely criticized. For example, see the book review by C. Wright Mills, on *The Social Life of a Modern Community*, in the *American Sociological Review*, April 1942; and more recently, H. W. Pfautz, and O. D. Duncan, "Critique of Warner's Work in Stratification," *American Sociological Review*, April, 1950. It is to be emphasized, however, that Warner and his associates, whatever the shortcomings of their research may be, are attacking a central

for the study of social stratification, and the explicit attention some of them have devoted to various aspects of the labor force participation, relatively little empirical research has been conducted by sociologists in this exceedingly important field.

In the various aspects of labor force participation, occupational affiliation has received the major attention as a key to social stratification—both as a direct measure of economic ranking—"class"—and as an index of honorific or social ranking—"status." Various writers have pointed to the importance of the occupation of a person as a basic factor in determining his role in the social order.<sup>28</sup>

There is no doubt that the occupation of a person is a fundamental factor in explaining his position and ranking in the economic and social order. A number of sociologists have discussed this problem, and some have used occupational data as a measure or index of rank in the economic and/or social order. A good introduction to relevant literature as well as challenging proposals for further research are contained in recent publications.<sup>29</sup>

problem in sociology which is suffering badly from lack of attention, and which calls for investigation by sociologists.

<sup>28</sup> Louis I. Dublin and Alfred Lotka, *Length of Life*, New York: Ronald Press, 1936, pp. 220.

Alba M. Edwards, *Comparative Occupation Statistics for the U. S., 1870-1940*, Washington, D. C.; Government Printing Office, 1943.

<sup>29</sup> Carlo I. Lastrucci, "The Status and Significance of Occupational Research," *American Sociological Review*, 11 (February 1946), 78-84.

William H. Form, and Delbert C. Miller, "Occupational Career Pattern as a Sociological Instrument," *American Journal of Sociology*, 54 (January 1949), 317-329.

Eldridge Sibley, "Some Demographic Clues to Stratification," *American Sociological Review*, 7 (June 1942) 322-330.

Paul K. Hatt, "Occupation and Social Stratification," *American Journal of Sociology*, 55 (May 1950), 533-543.

For examples of the use of occupational data for stratification purposes see: Alba M. Edwards, "A Social-Economic Grouping of the Gainful Worker in the U. S.," *Journal of the American Statistical Association*, 28 (December 1933), 377-387; *Comparative Occupation Statistics, 1870-1940*, Part III, U. S. Government Printing Office.

George S. Counts, "Social Status of Occupations: A Problem in Vocational Guidance," *School Review*, 33 (1925), 16-27.

Mapheus Smith, "Empirical Scale of Prestige Status of Occupations," *American Sociological Review*, 8 (April 1943), 185-192.

Social stratification will unquestionably remain one of the central problems in the study of social organization. As such it will continue to occupy a central place of interest in sociological research. There is ample opportunity and much need for research directed at improving occupational classification schemes for stratification analysis purposes; clarifying the role of occupation as a measure or index of "class" and of "status" respectively; determining the way in which, and the extent to which, occupation alone or in combination with other factors provides an efficient and effective measure of stratification; and ascertaining the extent to which, and the mechanisms whereby, the occupation of the person is, in fact, a determining factor in fixing a person's way of life.

As one aspect of the study of social stratification, the sociologist is usually interested in the problem of social mobility. Here, too, consideration of the labor force affiliation of the person, especially his occupation, points to important avenues of research. Utilization of labor force data for the study of social mobility, although increasing, is still in a relatively elementary stage. A number of interesting studies are available, however, and in addition to providing important findings, they also point to profitable further research.<sup>30</sup>

C. W. Hall, "Social Prestige Values of Selected Groups of Occupations," *Psychological Bulletin*, November, 1938.

Cecil C. North, and Paul K. Hatt, "Jobs & Occupations—A Popular Evaluation," in *Sociological Analysis* by Logan Wilson and William L. Kolb, New York: Harcourt, Brace & Co., 1949.

Richard Centers, *Psychology of Social Classes*, Princeton: Princeton University Press, 1949.

Also see forthcoming study by Paul K. Hatt, and Cecil C. North, reporting in greater detail results of the NORC study (No. 2044) resulting in an occupational rating scale to which reference is made in the citation above.

<sup>30</sup> P. Sorokin, *Social Mobility*, New York: Harper, 1927.

P. E. Davidson, and H. D. Anderson, *Occupational Mobility in an American Community*, Palo Alto: Stanford University Press, 1937; *Occupational Trends in the U. S.*, Palo Alto: Stanford University Press, 1940.

John W. McConnell, *The Evolution of Social Classes*, Washington, D. C.: American Council of Public Affairs, 1942.

Richard Centers, "Occupational Mobility of Urban Occupational Strata," *American Sociological Review*, 13 (April 1948), 197-203; also see W. H. Form, and Delbert C. Miller, "A Note on Occupational Mobility of Urban Occupational Strata, by

#### INDUSTRIAL SOCIOLOGY

Perhaps the most intensive sociological interest in the labor force is at present to be found, and will undoubtedly increasingly become manifest, in the emergent field of industrial sociology. An excellent statement of this interest is to be found in the editorial foreword to a special issue of the *American Journal of Sociology* devoted to industrial sociology:<sup>31</sup> "By publishing such an issue the Editors mean to recognize and to stimulate a growing interest in the study of institutions of work as forms of social organization which may be compared with others and be looked upon as a crucial part of the total organization of modern society and as the arena in which much of the individual's struggle for life, for a social self and personality goes on." This, to be sure, is a broader statement of the area of industrial sociology than is conceived by some students of the field who focus on industrial relations in the sense of labor-management relations;<sup>32</sup> or on the interaction of the two bureaucracies represented by corporate enterprise, on the one hand, and unionism on the other. But whether the field of industrial sociology be narrowly or broadly conceived, or,

Richard Centers," and reply by Centers, *loc. cit.*, 13 (October, 1948), 622-625.

W. H. Form, and Delbert C. Miller, "Occupational Career Pattern as a Sociological Instrument," *American Journal of Sociology*, 54 (January 1949), 317-329.

Richard Centers, "Marital Selection and Occupational Strata," *American Journal of Sociology*, 54 (May 1949), 530-535.

Carson McGuire, "Social Stratification and Mobility Patterns," *American Sociological Review*, 15 (April 1949), 195-204.

Natalie Rogoff, *Recent Trends in Occupational Mobility*, Ph.D. dissertation, University of Chicago, March 1950 (unpublished).

Eleanor H. Bernert, *The Chicago Labor Force, 1910-1940: A Comparative Study of Trends in the Labor Force and in Occupational and Industrial Characteristics*, Ph.D. dissertation, University of Chicago, 1949.

Gladys L. Palmer, and Ann Rattner, *Industrial and Occupational Trends in National Employment*, Industrial Research Department, Research Report No. 11, University of Pennsylvania, September, 1949.

<sup>31</sup> 54 (January, 1949), 28 ff.

<sup>32</sup> Herbert Blumer, "Sociological Theory in Industrial Relations," *American Sociological Review*, 12 (June 1947), 271-278.

Wilbert E. Moore, "Current Issues in Industrial Sociology," *American Sociological Review*, 12 (December 1947), 651-657.

for that matter, whether "industrial sociology" turns out to be an ephemeral sociological fad, it is clear that there is a beckoning and significant task for the sociologist in the explanation of the phenomena on which attention is focused in this field.

Turning first to the area of industrial relations, it is manifest that the sociologist has much to learn and much to contribute in the investigation of the structure and functioning of the labor force, as well as of management, from a cultural, institutional and social psychological approach. A critique of present approaches to this area of investigation, as well as a general formulation of the complex research problem involved, is contained in the paper by Herbert Blumer cited above (footnote 32). More detailed suggestions for research in this area, together with pertinent warnings about methodological inadequacies and the need for "analytical" rather than "practical" orientation<sup>33</sup> in the field, are contained in Moore's article, also cited above (footnote 32). Moore's suggestions center around types of research needed with respect to the organization of unions, and the consideration of collective bargaining and labor-management relations in general, as "the meeting of two bureaucracies."<sup>34</sup> Moore's cryptic reference to the field of industrial conflict as a "fertile area for the study of social disorganization—and for the analysis of the bases of functional integration in an urban industrial society"<sup>35</sup> may also be regarded as pregnant with sociological research potential.

It is particularly in industrial sociology that the sociologist can be expected to focus on work as a social institution and on the labor force participation of the person as basic research areas and as fundamental "behavior systems as a field for research" in the sense in which Hollingshead stated the problem.<sup>36</sup> Hughes, in following Durkheim's lead had attacked fundamental aspects of the problem

earlier.<sup>37</sup> A useful discussion of the broad significance and potentials of labor force research centered on occupation is to be found in the article by Lastrucci.<sup>38</sup>

Finally, opportunities for sociologically oriented research are to be found in the research proposals and activities of a number of economists and other students, particularly those in labor economics and industrial relations.<sup>39</sup>

### Conclusion

This essay is neither a comprehensive nor a balanced statement of the sociologists' interest, or of research opportunities, in the labor force.<sup>40</sup> It may, however, be useful in helping to focus attention, as a field for research, on a sector of contemporary life which is becoming increasingly complex, increasingly fraught with problems, and increasingly significant in the political as well as in the economic and social fields. These trends are concomitants of the increasing tempo of technological and social changes, initiated with the "industrial revolution" but still in progress and still producing new situations and new problems for which neither our social heritage nor our contemporary institutions provide ready or easy solutions. As an area in which many of our more difficult and perplexing domestic problems arise, the labor force is one which particularly merits the interest of the

<sup>37</sup> E. C. Hughes, "Personality Types and the Division of Labor," *American Journal of Sociology*, 33 (March 1928) 754-768.

<sup>38</sup> "The Status and Significance of Occupational Research," *American Sociological Review*, 11 (February 1946), 78-84.

<sup>39</sup> See: *Memorandum on University Research Programs in the Field of Labor, 1950*, Committee on Labor Market Research, New York: Social Science Research Council, 1950; also research planning memoranda published by the Committee on Labor Market Research of the Social Science Research Council, including:

John G. Turnbull, *Labor Management Relations: A Research Planning Memorandum* (Bulletin 61).

Gladys L. Palmer, *Research Planning Memorandum on Labor Mobility* (Pamphlet 2).

Carroll L. Shartle, *Vocational Counseling and Placement in the Community in Relation to Labor Mobility, Tenure and Other Factors* (Pamphlet 5).

Dale Yoder, *Demands for Labor: Opportunities for Research* (Pamphlet 7).

<sup>40</sup> For a fuller discussion of this entire subject see the forthcoming volume on the labor force by A. J. Jaffe and Charles Stewart, to be published by John Wiley & Sons.

<sup>33</sup> See also P. M. Hauser, "Social Science and Social Engineering," *Philosophy of Science*, 16 (July 1949), 209-218.

<sup>34</sup> See also Robert K. Merton, *Social Theory and Social Structure*, Glencoe: Free Press, 1949, pp. 151 ff.

E. W. Bakke, *Mutual Survival*, New York: Harper & Bros., 1947.

<sup>35</sup> *Loc. cit.*, p. 656.

<sup>36</sup> A. B. Hollingshead, "Behavior Systems as a Field for Research," *American Sociological Review*, 48 (December 1939), 816-822.



research-minded sociologist who is not perturbed or frightened by the fact that the problems are difficult, practical, and involve important conflicts of vested interests.

Valuable and significant as research opportunities of the various formal and quantifiable aspects of labor force structure and dynamics may be, the unique opportunity of the sociologist, both in contributing to a better understanding of labor force phenomena, on the one hand, and to a better understanding of our society, on the other, will lie in comprehensive research and analysis of work as a social institution, of industry and of the labor force as a fundamental sector of the social structure and process, and of labor force participation as a highly important segment of the person's behavior and way of life.

### SORORITY STATUS AND PERSONALITY ADJUSTMENT

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The question of desirability of fraternal organizations on college campuses has been widely discussed. On a few campuses, steps have been taken to eliminate the Greek organizations entirely. Of possible interest to those concerned

\*Acknowledgment is made to Dr. Helen Smith, Chairman, Department of Women's Physical Education, State College of Washington, who administered the personality test and prepared the original IBM cards.

with this problem is the study of comparative adjustments of sorority and non-sorority students made on the campus of the State College of Washington at Pullman in the Spring of 1942. At that time, the Bell Inventory,<sup>1</sup> a test which measures the home, health, social and emotional adjustment of the respondent, was administered to 864 women students.

The test was given to these students in their physical education classes, so the sample therefore includes all freshmen, sophomore, and junior women (since there is a three-year physical education requirement at the State College of Washington) except for the few who were absent from class when the test was administered and those who were excused from physical education. Many senior women not enrolled in physical education courses were willing to cooperate and were given the tests. The final sample includes 390 freshmen, 241 sophomores, 181 juniors, and 52 seniors.

Only three phases of the test—home, emotional, and social adjustment—are being dealt with in the following comparisons.

The test results for home adjustment showed that sorority girls were better adjusted to their home situation than were non-sorority girls (Table 1). Over half (52.8 per cent) of the sorority girls were well adjusted; as compared with only 44.9 per cent of the girls who did not belong to a sorority. At the opposite end of the adjustment scale, one-fourth of the non-sorority

<sup>1</sup> Hugh M. Bell, *The Adjustment Inventory—student form* (copyright by Stanford University, 1934).

TABLE 1. BELL HOME ADJUSTMENT SCORES OF STATE COLLEGE OF WASHINGTON WOMEN, CLASSIFIED BY SORORITY STATUS AND CLASS IN SCHOOL

	Well Adjusted		Average		Poorly Adjusted	
	Number	Per Cent	Number	Per Cent	Number	Per Cent
Freshmen and Sophomores						
(a) Non-sorority	195	45.8	119	27.9	112	26.3
(b) Sorority	104	50.3	74	35.7	29	14.0
Juniors and Seniors						
(c) Non-sorority	57	42.2	47	34.8	31	23.0
(d) Sorority	56	58.4	27	28.1	13	13.5
All Four Classes						
(e) Non-sorority	252	44.9	166	29.6	143	25.5
(f) Sorority	160	52.8	101	33.3	42	13.9
Critical Ratios						
(a) vs. (b)		1.06		2.00		3.49
(c) vs. (d)		2.43		1.08		1.81
(e) vs. (f)		2.22		1.12		3.97
(a) vs. (c)		0.73		1.53		0.77
(b) vs. (d)		1.31		1.31		0.12



girls were poorly adjusted, in contrast to only 13.9 per cent of the sorority girls. These differences were all statistically significant, as shown by the Critical Ratio test of significance.

The non-sorority girls who were freshmen or sophomores made a better showing in the "well adjusted" category than did the junior and senior non-sorority girls; among the sorority group a larger proportion of the upper-classmen were well adjusted to their home life than were the lower-classmen. These differences are not statistically significant, however, but the relationship seems quite clear. By applying the Critical Ratio test of significance to the difference of the differences in sorority status, between the upper and lower classes, it was found that there was a probability of 0.139 (C.R.=1.48) that such a difference, 11.7 per cent, would be observed. This is not statistically significant.

In emotional adjustment, the sorority members rated somewhat higher than the non-members, particularly the juniors and seniors (Table 2). The upper-class sorority girls were more often "well adjusted" emotionally than any of the other groups. The differences between sorority status groups are not statistically significant, but the differences between upper and lower classes are significant.

The Bell social adjustment scores showed even more striking differences between sorority and non-sorority girls (Table 3).<sup>2</sup> In both upper

and lower classes, the sorority members were more "aggressive" than the non-members. The proportion of socially "aggressive" non-sorority girls declined from the lower division to upper division classes, while the proportion increased (but not significantly) among the sorority girls. Fewer of the sorority girls were retiring than the non-sorority girls, the difference being very significant statistically. Here again, the Critical Ratio test of the difference of the differences in sorority status between the upper and lower classes was applied, and a probability of .0602 (C.R.=1.88) was found, which approaches statistical significance.

Whether the influence of sorority membership is responsible in any way for the superior adjustment realized by their members becomes the real problem. In order to solve it, one must know whether the sorority and non-sorority groups would have rated differently in personality adjustment at the time when the one group was accepted to sorority membership.

We have no measure of relative adjustment of the two groups at that time, but there is considerable evidence that the sorority pledges were selected from groups which are advancing, rather than good, average, and poor adjustment, as the home and emotional scores are divided. Students with very low scores, indicative of good adjustment on the home and emotional scales, show very aggressive traits on the social scale, while students with high scores are retiring. The in-between group, average, is considered the best socially adjusted group, with some aggressive traits being more desirable than retiring traits.

<sup>2</sup> The Bell social adjustment scores are divided into three divisions: aggressive, average, and retiring.

TABLE 2. BELL EMOTIONAL ADJUSTMENT SCORES OF STATE COLLEGE OF WASHINGTON WOMEN, CLASSIFIED BY SORORITY STATUS AND CLASS IN SCHOOL

	Well Adjusted		Average		Poorly Adjusted	
	Number	Per Cent	Number	Per Cent	Number	Per Cent
Freshmen and Sophomores						
(a) Non-sorority	129	30.3	179	42.0	118	27.7
(b) Sorority	66	31.9	87	42.0	54	26.1
Juniors and Seniors						
(c) Non-sorority	54	40.0	55	40.7	26	19.3
(d) Sorority	43	44.8	38	39.6	15	15.6
All Four Classes						
(e) Non-sorority	183	32.6	234	41.7	144	25.7
(f) Sorority	109	36.0	125	41.2	69	22.8
Critical Ratios						
(a) vs. (b)	0.41		....		0.42	
(c) vs. (d)	0.73		0.17		0.73	
(e) vs. (f)	1.01		0.14		0.94	
(a) vs. (c)	2.10		0.27		1.95	
(b) vs. (d)	2.17		0.39		2.03	

TABLE 3. BELL SOCIAL ADJUSTMENT SCORES OF STATE COLLEGE OF WASHINGTON WOMEN, CLASSIFIED BY SORORITY STATUS AND CLASS IN SCHOOL

	Aggressive		Average		Retiring	
	Number	Per Cent	Number	Per Cent	Number	Per Cent
Freshmen and Sophomores						
(a) Non-sorority	172	40.3	166	39.0	88	20.7
(b) Sorority	102	49.2	91	44.0	14	6.8
Juniors and Seniors						
(c) Non-sorority	49	36.3	65	48.1	21	15.6
(d) Sorority	51	53.1	33	34.4	12	12.5
All Four Classes						
(e) Non-Sorority	221	39.4	231	41.2	109	19.4
(f) Sorority	153	50.5	124	40.9	26	8.6
Critical Ratios						
(a) vs. (b)		2.12		1.20		4.46
(c) vs. (d)		2.54		2.08		0.66
(e) vs. (f)		3.14		0.09		4.17
(a) vs. (c)		0.83		1.87		1.31
(b) vs. (d)		0.63		1.58		1.65

tagged from the standpoint of personality adjustment.

First, as to place of residence: A majority, or 56.6 per cent, of the sorority girls came from cities of 10,000 or more compared with only 31.9 per cent of the non-sorority girls. Almost twice as many non-sorority girls as sorority girls came from farms (23.9 per cent compared to 13.6 per cent). Other aspects of our study show that city girls are better adjusted socially than are the farm girls. The city girls are also better adjusted emotionally. Practically no differences exist in the home adjustment of the residential groups.<sup>3</sup>

Second, as to occupation of the fathers: Using the Sims' classification of occupations<sup>4</sup> by socio-economic status (the classifications range from professional men and higher executives in Group I down to the unskilled laborers and peddlers in Group V), we found that a much larger percentage of sorority girls (24.2 per cent) than of non-sorority girls (9.7 per cent) came from families in which the breadwinner was in a Group I occupation. At the other end of the socio-economic scale, 19.5 per cent of the non-sorority and 10.9 per cent of sorority girls' fathers were in Groups IV and V (skilled and unskilled laborers). Fathers' occupation we found to be related to students' ad-

justment, the higher occupational classes having better home and emotional adjustment. On social adjustment, however, the lower occupational groups ranked almost as high as the higher occupational groups, with the middle occupational group showing the poorest adjustment.

Third, as to education of parents: Of the fathers of non-sorority girls, 27 per cent had had only a grade school education; of the sorority, only 9 per cent. At the other extreme, only a third of the fathers of non-sorority girls had had some college training, compared with half of the fathers of sorority girls. Comparable differences were found in mothers' education.

The daughters of the better-educated parents tended to be more "aggressive" in their social contacts, better adjusted emotionally and better adjusted to their home life.

Finally, as to the matter of the student's earning her own way, either wholly or partially, through college: As would be expected, judging by the socio-economic classification and education of the parents, more than twice as many non-sorority as sorority girls were at least partially supporting themselves; 18.5 per cent of the non-members were at least three-fourths self-supporting, as compared with 11.8 per cent of the sorority members.

In comparing girls who contribute to their own support and those who do not, we found the differences in adjustment slightly in favor of those who did not work.

These variations in the composition of sorority and non-sorority groups are undoubtedly

<sup>3</sup> For further data, see Paul H. Landis, "Personality Differences of Girls from Farm, Town, and City," *Rural Sociology*, 14 (March, 1949), 10-20.

<sup>4</sup> V. M. Sims, *The Measurement of Socio-Economic Status*, Public School Publishing Co., 1927.

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only a few of those which exist. However, they may serve as at least a partial explanation of the differences found in adjustment scores of the sorority and non-sorority girls.

In conclusion, it has been shown that the sorority girls showed some advantages over the non-sorority girls in the home, social and emotional adjustment as measured by the Bell Inventory. We know from their backgrounds that sorority membership is recruited from among the better adjusted. But the fact that the personality differentials which favored the sorority freshmen and sophomores are even greater among the juniors and seniors, though not statistically significant, suggests that sorority membership is probably a positive factor in personality adjustment.

#### RELATIONSHIP OF ABSENCE OF A PARENT TO COURTSHIP STATUS: A REPEAT STUDY

ROBERT O. ANDREWS AND  
HAROLD T. CHRISTENSEN

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Originally begun as an expansion of a portion of a lengthy study by Winch,<sup>1</sup> the research described below has become more a retest, in a new setting, of his findings. The results of the two investigations appear somewhat contradictory.

#### THE WINCH STUDY

In his study of the relations between loss of a parent and progress in courtship, it will be recalled that Winch measured degree of courtship behavior on an eight-point scale, then split the scale in the middle, categorizing the two portions as "high" and "low" levels of courtship.<sup>2</sup>

"High" level	"Low" level
Married	No dating
Formally engaged	Occasional dating
Not formally engaged, but have definite understanding to be married	Frequent dating
Have indefinite understanding that you will be married at some undefined time	Going steady

<sup>1</sup> Robert F. Winch, "The Relation Between the Loss of a Parent and Progress in Courtship," *Journal of Social Psychology*, 29 (1949), 51-56. This article covers only one small phase of a research which has been reported in several articles over a period of years.

<sup>2</sup> *Ibid.*, p. 53.

He divided his sample into three parts: (a) mother and father still living together, (b) father missing from home, mother did not remarry, subject living with mother, (c) mother missing from home, father did not remarry, subject living with father. Any person who had a parent missing at the time of administration of the questionnaire (1941) was included in a "parent missing" group. The three groups were tested separately for each sex. Subjects who did not fit into one of these groups were eliminated.

His research was carried out at sixteen coeducational colleges in the Middle West. His sample was composed of 495 men and 566 women, who were "white", non-Jewish, and between the ages of 19 and 25.

Winch found a significantly smaller number of males with father missing who fell in the "high" category than was true of males with both parents present. This difference was significant at the 5% level of confidence. No significant pattern was found for females.

#### THE RETEST

The present study used the Winch measure of degree of courtship behavior. The sample was composed of 1077 men and 626 women in Introductory Sociology and Marriage and Family Relationship classes at Purdue University between December 1949 and March 1950. This study is separated from that of Winch by the passage of nine years and the influence of a major war; the sex ratio at this University (focusing largely on engineering, agriculture and science) was about 6 to 1; many of the men in the present study were older (ages ranged from 16 to 37); a small (85) Jewish population is included. Except where otherwise noted, absence of a parent has been tabulated only when such absence began prior to high school graduation and extended up to the time the questionnaire was administered. "Parent absent" and "parent present" groups were based on Winch's criteria.

In the first phase of the investigation, the scale of degree of courtship behavior was split in half, as Winch had done earlier; i.e. "high" group was compared with "low" group by means of Chi-square analysis. For males, the trend was the same as Winch had found: those with father missing had the smallest proportion in the "high" group; next came those with both

parents present; and those with mother missing showed the largest proportion in the "high" group. However, where Winch had found the difference between males with father missing and each of the other two groups to be significant at the 5% level, none of the differences in

TABLE 1. RESULTS OF THE WINCH STUDY OF COURTSHIP STATUS AS RELATED TO ABSENCE OR PRESENCE OF PARENTS: MALES\*

Courtship Status	Father Missing		Both Parents Present		Mother Missing	
	N (1)	%	N (2)	%	N (3)	%
High	7	18	152	35	9	43
Low	32	82	283	65	12	57
Total	39	100	435	100	21	100

\* The chi-square test was applied to pairs of columns. Thus the analysis involved a series of fourfold tables. The value of Chi-square at a probability of .05 for one degree of freedom is 3.841.

Chi-square for columns one and two=4.63.

Chi-square for columns one and three=4.33.

The difference between columns two and three is not significant.

TABLE 2. RESULTS OF THE PRESENT STUDY OF COURTSHIP STATUS AS RELATED TO ABSENCE OR PRESENCE OF PARENTS: MALES\*

Courtship Status	Father Missing		Both Parents Present		Mother Missing	
	N (1)	%	N (2)	%	N (3)	%
High	37	44	411	45	13	.59
Low	47	56	493	55	9	41
Total	84	100	904	100	22	100

\* None of the differences is significant when tested by Chi-square analysis (5% level).

TABLE 3. RESULTS OF THE WINCH STUDY OF COURTSHIP STATUS AS RELATED TO ABSENCE OR PRESENCE OF PARENTS: FEMALES\*

Courtship Status	Father Missing		Both Parents Present		Mother Missing	
	N (1)	%	N (2)	%	N (3)	%
High	17	30	173	34	3	38
Low	39	70	329	66	5	62
Total	56	100	502	100	8	100

\* None of the differences is significant when tested by Chi-square analysis (5% level).

TABLE 4. RESULTS OF THE PRESENT STUDY OF COURTSHIP STATUS AS RELATED TO ABSENCE OR PRESENCE OF PARENTS: FEMALES\*

Courtship Status	Father Missing		Both Parents Present		Mother Missing	
	N (1)	%	N (2)	%	N (3)	%
High	21	57	239	43	3	27
Low	16	43	316	57	8	73
Total	37	100	555	100	11	100

\* None of the differences is significant when tested by Chi-square analysis (5% level).

the present study even approximate significance.<sup>3</sup>

After reaching this ambiguous conclusion, the second phase of the research was launched. An attempt was made to match each respondent in a "parent missing" group with one in the "parent present" group. The males were matched for age, university classification (Freshman 1, etc.), college residence (fraternity, co-op, etc.), religion, and size of home community.<sup>4</sup> Females were matched on all but the last factor, where no significant differences were found in the degree of courtship behavior ratings of the groups coming from the various rural-urban classifications used in the schedule.

Of the 84 males with father missing, 80 were matched. All 37 of the females with father missing were matched, as were the 22 males

<sup>3</sup> On the difference in findings, Winch has commented, "You may recall the underlying dynamics which I conceived as being consistent with my results is one in which the male has difficulty in achieving the appropriate sex type due to the lack of an identification figure. In terms of superficial behavior I should expect such a male to appear somewhat more 'feminine' than the average. It would be my hunch that Purdue would attract far fewer males of this type than would a liberal arts college. This hunch receives some measure of corroboration from the finding by Terman and Miles that an interest in engineering is a highly 'masculine' indicator. (Cf. *Sex and Personality*, McGraw-Hill, 1936.)" From a letter dated March 15, 1951.

<sup>4</sup> Each of the factors used for matching was tested against the six-point scale of degree (see below) of courtship behavior, and "breaks" significant at the 5% level were used for breaking each factor into categories. For instance, age categories used for males were 16-19, 20-21, 22-23, 24-25, 26 and over. For females, only two categories were used for matching age: 16-19, and 20 and over. Of course, the age range for males was much greater, since a larger percentage were veteran students.



with mother missing. Since there were only 11 females with mother missing, no attempt was made to match them, and no further tabulations were made on this latter group.

To attempt a finer discrimination than the dichotomous Chi-square comparison used in the preliminary analysis, a six-point scale was derived from the original eight-point measure of degree of courtship behavior. The scale was weighted arbitrarily.<sup>5</sup> The t-ratios of the means of experimental and control groups were then computed.

TABLE 5. THE MODIFIED WINCH SCALE AS USED FOR THE MAJOR PORTION OF THE STUDY, WITH THE WEIGHTS ASSIGNED TO EACH STEP

Weight	Step
6	Married
5	Formally engaged
4	Not formally engaged, but have definite understanding to be married
4	Have indefinite understanding that you will be married at some undefined time
4	Going steady
3	Frequent dating
2	Occasional dating
1	No dating

Even after matching and weighting, no significant differences (5% level) appeared between the degree of courtship behavior of the control and experimental groups. However, what appeared to be a meaningful pattern did emerge. The means of all three experimental groups were higher than those of their control groups.<sup>6</sup>

Intrigued by this development, the researchers pushed their inquiry back one more step. From the groups of those who had had a parent miss-

ing since the time of their high school graduation were abstracted those whose parent had been missing for a longer period, i.e., since the time of their leaving eighth grade. Here the same pattern of a higher degree of courtship behavior was apparent in the experimental groups. Of course, the samples involved here were somewhat smaller: 52 males with father missing, 27 females with father missing, and 14 males with mother missing. Again, none of the differences was significant at the 5% level of confidence.

#### DATING HISTORIES

Further analysis of the data on degree of courtship behavior appeared fruitless, so attention was turned to dating history data available on the original matched samples (groups with a parent absent at the time of high school graduation, and their control groups). The fourfold point coefficient method of comparison was used.<sup>7</sup>

Both males and females with father missing appear to follow a consistent pattern: they began dating earlier, first "went steady" at an earlier age, became engaged earlier and in larger numbers, and were more often involved in broken engagements than were those in the control groups. For the males (with the larger N) several of the differences were significant at the 5% level of confidence: age of first "going steady," age of first engagement, and proportion ever engaged. None of the differences for the females reached statistical significance but consistently followed the same pattern. Males with mother missing presented a more confused pattern, but in general followed that outlined above. None of the differences between this group and its control reached statistical significance.

#### CONCLUSIONS

The results of this study appear somewhat in contradiction to the earlier findings of Winch on the relation between absence of a parent and degree of courtship behavior of college students. Where he found the degree of courtship behavior to be retarded in the males with father missing, the present study appears to point in the direction of an accelerated courtship for both sexes

<sup>5</sup> The three categories, "Going steady," "Have indefinite understanding that you will be married at some undefined time," and "Not formally engaged, but have definite understanding to be married," were weighted equally. Not only did these categories seem less differentiating to the researchers than other categories used, but they also appeared somewhat synonymous to certain of the students who completed the schedule. In a similar manner, Dr. Winch had previously shortened his original nine-point scale by combining "going steady" and "keeping company."

<sup>6</sup> The method used was the t-test applied to two matched groups, as described in A. L. Edwards, *Experimental Design in Psychological Research*, New York: Rinehart, 1950, pp. 276-277.

<sup>7</sup> Since the samples involved are matched, the standard error was computed in a manner allowing for an expected correlation between the two samples, the four-fold point coefficient method described on pp. 87-89 of Edwards, *op. cit.*

when the father is absent. In neither study is a very clear picture presented of the effects of absence of the mother on the courtship behavior of either sex.

The opinion of the present researchers is that the scale of degree of courtship behavior used in both studies is not a very efficient measure of the effects of such absence on the courtship behavior of the offspring. One possible factor in its inefficiency is hinted by the analysis of dating histories in the present study: it appears that the courtship development of those with a father absent is somewhat accelerated, but that broken engagements are more numerous. In tabulating present degree of courtship behavior, therefore, we are not differentiating between those who were once engaged and now function at a "lower" level, and those who have never operated at a "higher" level.

The dating history data appear to be more illuminating. Where the father is missing, in the college population studied, the son or daughter seems to begin dating earlier, to go steady earlier, to become engaged at an earlier age (and in greater proportion), and to be more often involved in a broken engagement. Several of these differences are significant for the male, while none are for the female. However, the pattern is the same for both sexes.

In view of the apparent contradiction between this and the Winch study cited, further inquiry into the question seems indicated. The relative meaningfulness of the dating history analysis, as opposed to the more static measure of degree of courtship behavior, indicates to these researchers the need of a more dynamic approach in future inquiries, so that "relapses" are not averaged in with retardation of courtship progress.

A reasonable explanation of the findings of this study (if they be verified in more definitive research) would be that the offspring often desperately seeks an affectional relationship to replace that with the missing father. In the haste to effect such a relationship, not as much care is taken in making a choice as one would find taken by the average youngster from a "normal" home; thus broken engagements are more common. This is an exploratory explanation, of course. Why it does not apply equally to the absence of the mother is not even hinted by the present bit of research; however, the numbers involved here are extremely small.

The importance of testing such a hypothesis as the above seems self-evident, for it is not only in the home where the father is absent that offspring lack balanced affectional relation-

ships. If, as is hinted here, this is an important factor in incompatible mate choices, the relevance of any such additional knowledge to family literature becomes apparent.

#### COMMENT ON THE JUVENILE DELINQUENCY PREDICTION TABLES IN THE GLUECKS' *UNRAVELING JUVENILE DELINQUENCY*<sup>1</sup>

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*Harvard University Graduate School  
of Education*

*Unraveling Juvenile Delinquency*, by Sheldon and Eleanor Glueck, is a report of a ten-year study of juvenile delinquency. Although the entire research warrants careful critical examination, in this short paper we will examine only the approach the Gluecks have made to the prediction of juvenile delinquency.

One of the interests of the Gluecks is the identification, at the time they enter school, of those children who will later become delinquents. They comment on the importance of this problem as follows:

The selection of potential delinquents at the time of school entrance or soon thereafter would make possible the application of treatment measures that would be truly crime preventive. To wait for the certain manifestations of delinquency before applying therapy is to close the barn door after the horse has been stolen. Reliance on symptomatic behavior to select pre-delinquents is also a dubious procedure.<sup>2</sup>

It . . . becomes of crucial importance to develop prognostic instrumentalities that can be applied to children at the point of school entrance, without waiting for the actual appearance of serious and persistent antisociality. . . . With such an aid to the clinician and teacher, there is greater likelihood that the development of delinquency can be curbed than is possible under the present haphazard system. The success of such a preventive endeavor would naturally be dependent upon the expert clinical insights and resources available in school and community for the treatment of children found to be potential offenders. The school has the function not only of teaching the 'three R's' but of discovering and remedying those distortions of personality that are brought to the surface by the child's first attempts to adjust to the codes and

<sup>1</sup> Sheldon Glueck, and Eleanor Glueck, *Unraveling Juvenile Delinquency*, New York: The Commonwealth Fund, 1950.

<sup>2</sup> *Ibid.*, p. 257. The tables and quotations in this paper are reproduced by courtesy of The Commonwealth Fund.

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<sup>3</sup> *Ibid.*

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<sup>8</sup> *Ibid.*

authority imposed by adults outside the home. This stage in the child's contact with the adult world affords the acid test of his social adaptability; and if fundamental psychiatric, psychologic, educational, and social measures are not taken at this stage, subsequent remedial action becomes proportionately more difficult.<sup>3</sup>

The Gluecks have developed prediction tables which they believe can be applied at or soon after school entrance. They emphasize the point that these tables should not be used mechanically or as a substitute for clinical judgment. Since the services of trained social workers and psychologists, if not psychiatrists, are required to assemble the data on which predictions are made, many school systems will not be in a position to attempt the prediction of juvenile delinquency by the Gluecks' method. We believe that school systems which do have these professional services may wish to apply the prediction tables in Chapter XX of this book rather extensively. We therefore think it necessary to caution that the tables are valid only under such stringent conditions as to be of little utility at the present time.

For the purposes of their research, the Gluecks matched five hundred persistently delinquent boys from the city of Boston with five hundred non-delinquent boys. Matchings were made on the basis of residence in underprivileged areas, age, ethnic origin, and total intelligence quotient on the Wechsler-Bellevue Test. Previous studies by the Gluecks and others had shown that these variables were related to delinquency in a significant manner. By controlling these variables, the Gluecks were able to study the relationships between delinquency status and numerous family, community, school, and psychological variables independent of the matching variables. Although their research de-

sign is appropriate for tests of significance of differences between the delinquent and non-delinquent groups, it is not appropriate for the prediction of delinquency and non-delinquency. The non-delinquents in this study are not representative of the general population of non-delinquents from which they are drawn. As is indicated in Tables IV-2, IV-3, IV-6, and IV-7,<sup>4</sup> the matching procedure resulted in the selection of a sample of non-delinquent boys who are below the general average of intelligence, who are from underprivileged areas where high delinquency rates prevail, and who are not representative of the ethnic distribution of the general population.

In developing the prediction tables reported in Chapter XX, the Gluecks have employed a technique which they had previously used in their book, *500 Criminal Careers*.<sup>5</sup> For the first of these tables, Table XX-2,<sup>6</sup> five family and personal history factors were selected in which the delinquent and non-delinquent groups had been shown to be significantly different. These factors were discipline of boy by father, supervision of boy by mother, affection of father for boy, affection of mother for boy, and cohesiveness of family. A weighted failure score, the per cent of boys in a given category of a factor who were delinquent,<sup>7</sup> was determined for each of the categories within a factor. The sum of the weighted failure scores on the five factors was then obtained for each boy studied. These scores were distributed as shown in Table XX-2.

<sup>4</sup> *Ibid.*, pp. 36-38.

<sup>5</sup> Sheldon Glueck, and Eleanor T. Glueck, *500 Criminal Careers*, New York: Alfred A. Knopf, 1930.

<sup>6</sup> Glueck and Glueck, (1950), *op. cit.*, p. 261.

<sup>7</sup> These scores are subject to the same criticism which we direct at their sum. However, this would not be a serious deficiency if the sum were valid.

<sup>3</sup> *Ibid.*, p. 258.

TABLE XX-2. DETAILED PREDICTION TABLE FROM FIVE FACTORS OF SOCIAL BACKGROUND<sup>8</sup>

Weighted Failure Score Class	Number of Delinquents	Chances of Delinquency (per hundred)	Number of Non-Delinquents	Chances of Non-Delinquency (per hundred)
Under 150	5	2.9	167	97.1
150-199	19	15.7	102	84.3
200-249	40	37.0	68	63.0
250-299	122	63.5	70	36.5
300-349	141	86.0	23	14.0
350-399	73	90.1	8	9.9
400 and over	51	98.1	1	1.9
TOTAL	451		439	

<sup>8</sup> *Ibid.*, p. 261.

TABLE IV-2. DELINQUENCY RATE OF AREAS IN WHICH BOSTON DELINQUENTS AND NON-DELINQUENTS WERE LIVING WHEN SELECTED FOR STUDY<sup>9</sup>

Delinquency Rate	Delinquents		Non-Delinquents	
	Number	Per Cent	Number	Per Cent
Less than 10 per 1,000	26	5.8	27	5.4
10-24.9 per 1,000	267	59.2	272	54.6
25-49.9 per 1,000	90	20.0	113	22.7
50-100 per 1,000	68	15.0	86	17.3
TOTAL	451	100.0	498	100.0

<sup>9</sup> *Ibid.*, p. 36. This table is not reproduced in full.

This table can be criticized on several grounds. Use of the same data from which part scores are derived to study the validity of their sum maximizes the effect of chance elements. In addition, inspection of the table reveals, as is expected from the sampling procedure, that the number of delinquents is roughly equal to half of the total sample. Because of this restriction, the table is valid for only those populations in which the number of delinquents is approximately 500 per 1000. We know from Table IV-2 that the delinquency rate in the population from which the Gluecks drew their samples is considerably less than 500 per 1000, as shown in Table IV-2.

In order to introduce approximations into

Table XX-2 which will remove the restriction that the delinquency rate is approximately 500 per 1000, it is necessary to know the fraction of a given six-year-old population which will be delinquent, in the Gluecks' definition of the word, by age seventeen. It is difficult to make an estimate of the average delinquency rate of the population sampled by the Gluecks. The rates in the above table include girls as well as boys, and are based on the number of arrests in a given period rather than on individuals arrested. However, Shaw and McKay<sup>10</sup> report

<sup>10</sup> Clifford R. Shaw and Henry D. McKay, *Juvenile Delinquency and Urban Areas*, Chicago: The University of Chicago Press, 1942.

TABLE A. PREDICTION OF DELINQUENCY FROM THE GLUECKS' FIVE FACTORS OF SOCIAL BACKGROUND PRESUMING A DELINQUENCY RATE OF 70 PER 1000

(An Adaptation of the Gluecks' Table XX-2)

Weighted Failure Score Class	Number of Delinquents	Chances of Delinquency (per hundred)	Number of Non-Delinquents	Chances of Non-Delinquency (per hundred)
Under 150	5	0.2	2279	99.8
150-199	19	1.3	1392*	98.7*
200-249	40	4.1	928	95.9
250-299	122	11.3	955	88.7
300-349	141	31.0	314	69.0
350-399	73	40.1	109	59.9
400 and over	51	78.5	14	21.5
TOTAL	451		5991**	

\* *Sample calculation:* There will be 93 non-delinquents for each 7 delinquents. This is a ratio of 13.285714 to 1. The total number of non-delinquents with whom the 451 delinquents are compared will be  $451 \times 13.285714 = 5992$ . In the Gluecks' sample, 102 of the 439 non-delinquents had a weighted failure score in the 150-199 class, thus there will be  $\left(\frac{102}{439}\right) 5992 = 1392$  non-delinquents in this class in the expanded group. The number of delinquents in this score class is 19; therefore the per cent of non-delinquents in this class in the expanded group will be  $\left[\frac{1392}{1392 + 19}\right] 100 = 98.7$ .

\*\* Because frequencies estimated in each class were rounded to whole numbers, their sum is 5991 instead of the more accurate 5992.



that, in the city of Boston as a whole, seven boys per hundred between the ages of 10-16 committed serious offenses during the three-year period from June 1, 1927 to May 31, 1930.

If we *assume* that currently seven out of one hundred boys of age six in the city of Boston will commit serious offenses at some time between the ages of 6 and 17, and if we *assume* that the weighted failure scores of the non-delinquents will be distributed in proportion to those of the non-delinquent boys in Table XX-2, the chances of delinquency and non-delinquency are shown in Table A on page 546.

Since the Gluecks' sample of non-delinquent boys is a select group, it is quite possible that the weighted failure scores of an unselected sample of six-year-old boys in the city of Boston will not be distributed as it has been necessary for us to assume in Table A. It seems likely to us that there would be a larger proportion of low scores in an unselected group of non-delinquents, but we are not able to provide estimates of this different distribution.

Within the framework of assumptions we have stated, it is possible to estimate the predictive efficiency of the Gluecks' data when a reasonable approximation of the actual delinquency rate is used in the construction of the prediction table. In Table A we see that only in the 400-and-over class of weighted failure scores are more than half of the boys delinquent. Even in this class, errors will be made in 21.5 per cent of the cases if everyone scoring 400 and over is predicted to be delinquent. If we make the prediction that every boy in the expanded group will be non-delinquent, we will make 451 errors. If we predict that every boy with a weighted failure score of 399 or less will be non-delinquent, and all over 399 delinquent, we will make 414 errors. This is not a very significant reduction in error, and the choice of a critical score at any other point on the table leads to a greater number of errors.

Table XX-2 of the Gluecks presents quite a different picture. In this table, if we predict that every boy will be delinquent, we will make 439 errors; this is a smaller number of errors than we will make if we predict that every boy will be non-delinquent. The smallest number of errors will be made if all boys with weighted failure scores of 249 and less are classified as future non-delinquents, and if all boys with scores of 250 and above are classified as future delinquents. In this case we would misclassify 64 delinquents and 102 non-delinquents, a total of

166 errors, or approximately 17 per cent of our predictions.

In the above analysis we have used the idea of maximum likelihood in making predictions, i.e., of classifying as a delinquent any boy whose chances of being delinquent are greater than 50 in 100. Because of the premium our society places on the prevention of juvenile delinquency we may desire to use Table A in a less restrictive fashion. If we give special attention to all boys scoring 350 and over, we will need to consider about 4 per cent of the entire group and only about one-half of these would, in the ordinary course of events, be delinquent. Special attention given to boys scoring 300 and over will involve about 11 per cent of the group, and only about 38 per cent of these will be potential delinquents. It should be noted, however, that, at the expense of giving special attention to around 11 per cent of the entire group, about 59 per cent of all the potential delinquents would be under surveillance. The ratio of delinquents to non-delinquents increases so rapidly for lower weighted failure score classes that it seems impractical to attempt to provide special attention for boys scoring 299 or less.

The Gluecks' Tables XX-6<sup>11</sup> and XX-7<sup>12</sup> give essentially similar results.

In discussing their prediction tables the Gluecks assume that criteria established empirically from a sample of boys with an average age of about fourteen and a half years can be used to predict future behavior of six-year-olds. It does seem possible that the factors of social background used in the construction of Table XX-2 might be the same at six as at fourteen. However, we do not share the Gluecks' faith in the stability of the Rorschach information summarized in Table XX-6 and the personality trait ratings summarized in Table XX-10. We do not know whether ratings like these made on boys after the age of six would be similar to ratings on the same individuals at age six or not. These assumptions need further exploration.

In the construction of prediction tables in Chapter XX, the Gluecks have assumed that this investigation is similar to other investigations in which they have constructed prediction tables. For instance, in *500 Criminal Careers*<sup>13</sup>

<sup>11</sup> *Op. cit.*, p. 263.

<sup>12</sup> *Op. cit.*, p. 265.

<sup>13</sup> Glueck and Glueck, (1930), *op. cit.*

the Gluecks reported upon an investigation of 510 former inmates of the Massachusetts Reformatory. In this study the tables for predicting recidivism which they developed were not subject to arbitrary selection of the number of criminals in the three classes of parole success which they used, a restriction which has been placed on the prediction tables in *Unraveling Juvenile Delinquency*. The earlier tables are based on reasonable estimates of the relative frequency of criminals on parole who after five years would fall into the categories, "success," "partial failure," and "total failure." In this case, dividing the number of prisoners in each of the parole success categories who fell into a particular total failure score class by all the parolees in this score class actually provides an estimate of the chances of parole success associated with that score class.

In their basic comparative studies the Gluecks

have isolated many variables which should be useful in the prediction of juvenile delinquency. However, because of their sampling design, their prediction tables cannot be used in most school systems without modification. The prediction studies of the Gluecks represent only the first stage in the development of prediction data: the establishment of criteria. After this, attention should be given to the problem of combining the criteria in the most efficient manner. The Gluecks' method is not the most efficient, but its practical advantages cannot be ignored. The next stage, the application of the criteria to an independent second sample, may have to be followed by a study of the sources of error if prediction accuracy proves to be low. Like most prediction problems, juvenile delinquency prediction requires long term longitudinal study. We hope that the Gluecks' work will encourage initiation of such studies in schools.

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## COMMUNICATIONS AND OPINION



### ON THE USE OF QUESTIONNAIRES IN RESEARCH: OPEN LETTER TO A GRADUATE STUDENT

Dear Sir:

In reply to your letter and questionnaire concerning your research project, I would like to give you my candid reaction to the kind of procedure which you propose to use. I assume that your major professor knows about your questionnaire and I want him to read this letter. If your request were singular in its kind, I might not take the trouble, but it seems to represent a pattern of graduate "research" technique which is spreading—a pattern which in my opinion involves not only unnecessary impositions on the time of busy university professors, but also a decline of scholarly standards.

First of all, you are asking in the questionnaire certain questions you could easily answer for yourself by consulting *Who's Who in America* or the university catalogues.

More serious is the objection which I have to the rest of your questions; you are asking me for my opinions on very complex questions, and you formulate your questions in a way that indicates you expect a dogmatic answer. To do real justice to these questions, which concern the objectives and methods of . . . . . sociology. I would have to write you an essay, or several papers. It is hard to imagine that you really expect me to do this for you; if you do not, then why ask me these questions? Furthermore, it so happens that I have expressed my ideas on these matters in several publications; I admit that my opinions are in some cases not stated explicitly but by implication. Now there is an old and well established way of getting information about other scholars' opinions and theories; that is, by reading and by critical interpretation. There is no substitute for this. My advice to you is to forget about the questionnaire and to study the literature.

Very sincerely yours,  
RUDOLF HEBERLE

Louisiana State University

### ON BIERSTEDT'S REVIEW OF POWDER-MAKER'S *HOLLYWOOD—THE DREAM FACTORY*

To the Editor:

In a recent address to anthropologists I chided them for slighting the work, methods, and theoretical structures of sociology. Among other things I pointed out that the *American Sociological Review* reviews many more anthropological books than does the *American Anthropologist* review sociological works.

In speaking to an anthropological audience I did not point out that frequently the reviewing of anthropological works in the *American Sociological Review* apparently is regarded as an opportunity for bright young sociologists to exhibit their flair for invective and sarcasm. This is particularly true when an anthropologist takes a look at some aspect of American culture, even though it be one where even the sociological characteristics have been neglected or ignored. In such cases reviewers frequently not only demonstrate the sharpness of their rapiers (which sometimes turn out to be clumsy clubs) but are both shrilly indignant at the invasion of their preserves and fail to recognize that there is any difference between a cultural and societal approach to phenomena. Certainly they are close to unanimous in their feeling that no good can come of the occasional incursion of anthropologists into complex cultures. As one who has long advocated more research by sociologists outside the industrial North Atlantic cultures, I find the persistence of isolationist attitudes in any field of social science to be a disturbing mark of our immaturity.

These thoughts were brought to a head by Robert Bierstedt's review of Hortense Powdermaker's recent book on Hollywood.<sup>1</sup> In this case I have some personal involvement as I played a small part in making it possible for

<sup>1</sup> *Hollywood—The Dream Factory: An Anthropologist Looks at the Movie Makers*, Boston, 1950. Reviewed in *American Sociological Review*, 16 (Feb. 1951), 124-125.

Miss Powdermaker to extend her stay in Hollywood and had some minor consultative function (along with many others) in aiding her investigation.

During his all too brief membership on the Social Science Research Council, Robert Oppenheimer once suggested that one reason for the low esteem in which the social sciences are held is our low opinion of ourselves. In the physical sciences according to Oppenheimer (I think perhaps he overstated his case) mistakes are taken for granted on the part of even the best scientists and the profession searches for that which may be useful and which may contribute to additive research and the building of the science. In the social sciences, in contrast, he felt that we sit around sharpening scalpels to dissect, damn, and discard the product of anyone courageous enough to put his results on paper. To suggest either ways of verification of dubious results or further steps for future research is relatively much rarer.

This is not an argument that reviewing should all be sweetness and light. Weaknesses in methodology, conceptualization, and presentation of results should certainly be indicated, although the reviewer should be sure he knows or understands the basic premises and objectives. Certainly from the tenor of his review Bierstedt would answer that the Powdermaker book is entirely lacking in utility, but what purpose is served by lengthy listings of analogies (good or bad) and grammatical errors is obscure to me.

Certainly I am not prepared to defend Miss Powdermaker's somewhat ill-advised and often hastily prepared attempt at a popular report on her findings with its accompanying factual errors. It is unfortunate also that she does not make more explicit the scope of her research material instead of quoting so extensively for her popular audience from the readily accessible *Variety* and *New York Times* (although this is fairly clearly indicated in her introductory chapters which apparently no reviewer has bothered to read). Neither does she suggest the larger purposes of her investigation (as opposed to this particular product) and the way it fits into her research program. Nevertheless, it seems to me her present book is entitled to be reviewed in terms of her immediate and limited objectives.

One measure of the success of Miss Powdermaker's purpose is the degree to which her book elicited violent reactions from the trade journals. Yet it is from these that Bierstedt takes the tone of his review. It may be true that

"Most of it could have been put together by any hep Hollywood correspondent in two weeks" as Bierstedt quotes from the *Variety* review; one wonders why no one has done so. (Any fool could have told Newton that apples fell.) The anthropological analogies are certainly often labored and the editorial work of the publisher is inexcusably slipshod. What really relieves those of us who have lived on the uneasy fringes of Hollywood, though, is the reviewer's dictum that the book produces no new information and the implication that the power structure and jockeying for position are no different from any other business (or academic) situation. Some of us had the uneasy feeling that Hollywood was something special; instead, it is just another bank—or university.

When I started writing I did not intend to single out Bierstedt's review for such particular criticism. The small spark which really motivated this letter (I believe this is my first "letter to an editor") is Bierstedt's identification of symbiosis as a sociological concept. Anyone familiar with the biological roots of anthropology would realize both the origin of the concept and the fact that few if any anthropologists would learn it from sociology or use it in the sociologist's sense.

RALPH L. BEALS

*University of California at  
Los Angeles*

#### COMMENT ON WALTER COUTU'S "ROLE-PLAYING VS. ROLE- TAKING"\*

*To the Editor:*

In the name of the many who in the last thirty years have painstakingly worked to establish piecemeal the beginnings of an experimental sociology and sociometry we question here the value of sociological writing which takes us back into an era in which definition by statement and subjective conviction was the rule, a condition which we have tried so hard to overcome. It is a regression to a period in which one system of concepts was placed opposite the other, one definition versus the other, without any further proof except the intuitive honesty of the writer and his good

\* Walter Coutu, "Role-Playing vs. Role-Taking: An Appeal for Clarification," *American Sociological Review*, 16 (April 1951), 180-187.



naturedness, the persuasiveness and perhaps the brilliance of his style. It is against this sociology *ex cathedra* that these comments are directed.

The term role itself comes from the language of the stage. Role-playing started out as an experimental procedure, as a practice, a method of learning for performing roles adequately. The present popularity of term and concept derives from the value it has proven to have as a training device in various social, occupational and vocational activities. It is through the study of roles *in action* that the new knowledge about roles developed. The present vogue of role-playing resulted from the initiative which we have taken in developing them. In contrast to role-playing, role-taking is an attitude already *frozen* in the behavior of the person. It is not an act, not a play, it is a finished product, a role conserve.

I agree with Coutu that Mead's concept of role-taking and the concept of role-playing as developed by socio- and psycho-dramatists mean two different approaches. However, I disagree with Coutu when he tries to separate role-taking and role-playing as if they would be different entities, defining role-playing as a sociological concept and role-taking as a psychological concept. Concrete role research indicates clearly that there is no such radical discreteness of concept. Coutu's definitions of role-taking and role-playing are a fine illustration of the confusion into which aprioristic, logical play with concepts can take us. There is a consensus in all studies made that role-taking and role-playing have a common origin. The genesis of role development shows clearly how one grows out of the other, that role-playing and role-taking are two phases of the same process. Nothing is gained at this stage of role research by separating them, postulating one as psychological, the other as sociological. It has been found in hundreds of try-outs that the process of role-taking is not only cognitive and that, on the other hand, the process of role-playing is not only behavior or mere acting, but that cognition, perception, behavior and action are finely interwoven and cannot be neatly separated. It has been found, in the study of children, that there are enactable and unenactable roles; recognized and unrecognized roles; enactment of roles before the level of their recognition; recognition of roles before the level of their enactment; adequate, distorted, partial and loss of role perception; adequate, distorted, partial and inability of role enactment. There is often a discrepancy between the assessment of role

behavior by observers and the assessment of such roles in action by the actors and co-actors themselves (See *Sociometry*, Vol. 8, 426-441, 1945). However much taken and frozen a role has become and however much integrated it is into the perception and behavior of a certain individual, there is a weak spot in its armor; in order to emerge in a certain moment it must pass (a) through a process of warming up, however minimal, in which the whole organism is involved, (b) a process of mimetic learning as to how to take the role of the other—however "generalized" this "other" may be. The individual represents every time a slightly different version; this is not possible without some minimal playing towards the role, gradually learning and struggling to approximate it—however fragmentary, rudimentary and embryonic this role-playing process might be. Role acting and role perception, role-playing and role-taking go hand in hand in the primary learning and conditioning process. *In situ* they cannot be separated.

Mr. Coutu is right in asking for consistency of definition. But arbitrary definitions are not necessarily helpful, especially when they tend to make over-discrete what is not discrete, as in the case of role-playing and role-taking. It may be that just the inconsistency of usage of the terms by Newcomb, Sargent, Lindesmith and Strauss indicates a closer sensitivity for the actual facts than the logical plea for precision of Coutu.

J. L. MORENO

*Sociometric Institute,  
New York, N. Y.*

#### COMMENT ON HART'S "SOME CULTURAL-LAG PROBLEMS WHICH SOCIAL SCIENCE HAS SOLVED"

*To the Editor:*

In the past issues of the *American Sociological Review* ample space has been given to Dr. Hart's trend lines, logistic curves, and to his Comteian value orientation that everything is going to turn out all right if the social scientists would only work a little harder, faster, and apply their findings. The last contribution of Dr. Hart's in the April 1951 issue epitomizes the faults of this science-building activity. The objections are logical, factual, and moral, and especially the last if the reader were a member of a minority group. In fact, he may wonder

what sort of a science it is that tells him everything is coming along fine, a point to be discussed below.

Dr. Hart attempts to show that social science has solved some cultural-lag problems. This proposition has three aspects. The first relates to the status of the culture-lag theory with its correlated notions of social and cultural organization and of change. Since the acceptance of this theory is still uncertain and controversial, Hart's entire discussion will be affected by this debatable theory. Taking the idea in its simplistic form—material and/or technological culture changes at a faster rate than the social, I think his examples do not fit the notion and in fact demonstrate the reverse. For example, the development of cities is associated with a high typhoid death rate. The problem is solved via the findings of bacteriology and sanitary engineering. In this case the "material culture" had to catch up with the new social form. The same seems to hold true for his example of infant death rates due to diarrhea and enteritis. If these interpretations are correct, Hart's ideas are contradictory, and his case falls down. The second problem is one of classifying the problems as culture-lag problems. Five of these problems, he admits, are mainly engineering and biological problems, and the solutions originate in these technological sciences rather than in the social. Hart does not show that the public application, i.e. the correlative social organization to apply these technical solutions, was instrumentalized by the social sciences. The assumption that social science has been mainly or even partially a factor in the solution of the six problems is a gratuitous one. No data were presented to demonstrate this third aspect, *viz.*, that the social sciences solved the problems. Furthermore, the data presented introduce a contradiction.

Another problem relates to the inferences that may be justifiably drawn from a trend line. Is it correct to infer from a decline in lynching that Negroes are better off? Hart draws that inference *after* assuming the existence of another context, one that supports his case. The contexts to which these atomistic trend lines are related are handled in a rather *ad hoc* and cavalier fashion, e.g. his discussion of the social conquest of lynching.

In what ways lynching represents an instance of culture-lag is rather unclear to me since it is essentially a matter of social relations. Without going any further into this aspect, I think that Hart's conclusions are a distinct disservice to a

minority problem. They are vitiated by his value assumptions. The decline in lynching is taken as evidence that Negro deaths by violence are on the wane. This decline he attributes to the "development of greater understanding, tolerance, and readiness to accept law and order." No proof is offered for this assertion. Since lynching after 1900 is mostly a matter of Negro deaths, what about Negro deaths, and furthermore, what about this greater understanding and tolerance? The situation of a minority group must be looked upon as a whole and not in the atomistic fashion of Hart's. In the following discussion I am not including hate strikes by workers and by elementary and high school students (including some rioting here), police brutalities, the recurring cases of peonage, and the new trend in the South of justifiable homicide verdicts rendered by all-white juries and white coroners. What is significant is that the individualized pattern of lynching in more or less rural areas has been superseded by mass violence against Negroes and other minority groups in urban areas. Negroes and other minority group members are killed and beaten up on the grand scale.

The total number of riots for a six-year period are given in Table 1; 65 persons were killed, and of these, 40 were Negroes. The total number of lynchings reported 1937-1947 came to 42. Over half of the Negroes killed in the riots were killed in the 1943 Detroit riot. The number of Negroes seriously wounded or otherwise beaten up is difficult to estimate, but the estimate for the Detroit and Harlem riots came to 961 persons. In all likelihood these figures are underestimated. Property damages for these two riots were estimated at \$2,225,000. The 1935

TABLE 1

NUMBER OF REPORTED CIVILIAN AND MILITARY RIOTS, 1942-1947

Type of Riot	Year						
	1942	1943	1944	1945	1946	1947	Total
Civilian	1	11	—	—	4	1	17
Military	9	8	—	1	2	2	22
Total	10	19	—	1	6	3	39

Source: *Negro Handbook*, 1944, 1949, Florence Murray, ed., New York: The Macmillan Co.; *The Negro Yearbook*, 1947, Jessie Guzman, ed., Tuskegee Institute. Military refers to riots between Negro soldiers and white military or civilian police.

Harlem riot resulted in an estimated four million to five million dollar property damage. In Columbia, Tenn., the Negro ghetto was completely shot up, ransacked, and pillaged. The social consequences of these expressions of mass violence cannot be estimated. Since race hatred and race baiting still continue in these centers and in others are fostered by public relations agencies, e.g. the defeat of Senator Graham in a recent North Carolina election, the minority situation is as explosive as ever. "The systematic use of social intelligence" has been indeed very effective. The neo-postivistic answer of Hart's is false and misleading. The decisive role of the social sciences in conquering death or culture-lag problems is unproven.

H. OTTO DAHLKE

Ohio University

# COMMENTS ON THE REVIEW OF STUDIES IN LEADERSHIP

To the Editor:

I suspect that, deep down, every editor or author dreams of the time when he can demonstrate that a hostile reviewer has actually not read his book. Usually, this is a very difficult thing to do convincingly. I believe, however, that it can be definitely established that Lyford Edwards' review of *STUDIES IN LEADERSHIP* (A. S. R., June, 1951, p. 416) was not actually based upon a reading of the book and, moreover, that it is largely a concoction of irrelevancies and ambiguities.

Edwards' own words provide clear-cut evidence that he has violated the most fundamental duty of a reviewer, which is to read the book and not the blurb. The blurb on the book's dust jacket states, "Four main types of leaders are discussed: bureaucrats, agitators, *intellectuals*, and informal leaders." (All italics in this comment are my own.) Mr. Edwards echoes the blurb by writing that Part I of the volume "deals sketchily with bureaucrats, agitators, *intellectuals*, and informal leaders." I say that he echoes the *blurb*, not the book, because nowhere in the book will you find an article on the intellectual as a leadership type.

Moreover, the *blurb* reads: "The discussion of political leadership ranges all the way from the local ward-heeler up to the president . . ." Merely reversing the order, Edwards states that the book contains articles on political leadership "ranging all the way from president

down to ward-heelers." The crucial point is this: Nowhere in the book is there a single article which examines the presidency!

It is true that I had *intended* to have articles dealing with the presidency and with intellectuals as leaders. At the last moment, however, considerations of space compelled me to omit these and other articles. But it was too late to change the dust jacket's description of the book's contents. For this reason *the blurb itself is in error*. And it can only be because Edwards relied upon the blurb, not the book, that his description of the book's contents has *exactly the same errors*. (He also repeats two other errors found in the dust jacket, but considerations of space forbid examination of these.)

Edwards maintains that the volume deals "sketchily" with the types of leaders. Let us omit consideration of the fact that you cannot deal sketchily with a leadership type—intellectuals—that you have not touched upon at all. What should be indicated is that all of Part I is devoted to this question, and it covers some 75 pages. Say what you will, 75 pages by Robert Merton, Paul Lazarsfeld, Leo Lowenthal, William F. Whyte, and Bernard Berelson are not a "sketch" in anyone's language. If Mr. Edwards thought there were other articles, somehow superior to these, which should have been included, then why didn't he get down to cases and state specifically which ones?

Concerning irrelevancies: Edwards states, "There are no women represented except two co-authors." True—but does this make the book good or bad? What on earth does the sex of a social scientist have to do with judgments of his work? If Mr. Edwards meant to imply that I discriminated against women in choosing articles for inclusion, then how does he explain that this volume, unlike most others on leadership, had a special article on leadership problems among women (see the paper by Arnold Green and Eleanor Melnick).

Further irrelevancies: "For the most part," asserts Mr. Edwards, "the writers are not well known, which says nothing against their technical competence." Are the contributors really so unknown? About half of them have published technical volumes which are recognized as good social science, and almost all of them have published articles in technical journals. At the University of Buffalo, even an undergraduate could not hope to pass his sociology orals unless he knew Oliver Cox, Leonard Doob, Alfred McClung Lee, Kurt Lewin, Seymour Lipset,



Philip Selznick, W. Lloyd Warner and the others previously mentioned.

Part III of the book, writes Edwards, "presents some techniques by which it is alleged (*sic*) authoritarian leaders can be controlled within a democratic society. *These sum up to responsibility to the electorate.*" Actually, Adorno, Doob, and Kutner discuss *concrete* techniques—e.g., a new type of pamphlet, semantic analyses, institutionalized periods of self-criticism—for controlling authoritarian leaders. These no more "sum up to responsibility to the electorate" than psychotherapy sums up to the Golden Rule.

Throughout, Edwards never makes a concrete criticism of an idea, technique, or proposition. All he can say is things are "too sketchy," "casual," "not clearly brought out." Such judgments can never be refuted, they can only be sympathized with.

Typical of his review is the following vague paragraph: "Part IV is concerned with the ethics and techniques of leadership. After some observations on mass apathy, there are proposals for democratic participation and followership. The subject cannot be properly treated without more knowledge of casuistry than any of the writers possess. A reasonably competent Jesuit could do twice as well in half the space." As a reverend doctor, Edwards probably knows what Jesuits can do, or does he? But since it is not quite clear whether he maintains that the contributors are worse than or better than Jesuits, all one can do is to bow to those—apparently like Mr. Edwards—who are true masters of casuistry.

Edwards concludes his review with a "D-Day" assault on my "one deadly defect." (If he knew me well enough, I am sure he would find others.) The Deadly Defect is, of course, an "unreadable style," which I presumably share with the "majority of sociologists." For my part, I am ready to admit that my style can

stand *considerable* improvement, along with that of many other social scientists. But Mr. Edwards' indiscriminating tirade against the book's style, and the style of sociologists at large, is an expression of an overriding antipathy rather than a reasoned judgment. Robert Nisbet's article, for example, was not only readable but, in the opinion of the *San Francisco Chronicle* (Jan. 28th, 1951), was "eloquently" written. Lowenthal and Guterman's style is, at the very least, stimulating and often moving. The selections from William F. Whyte's *Street Corner Society* are, moreover, known to the entire trade as models of informal clarity. Sure there was some turgid stuff, and part of it undoubtedly written by myself. Readability though, like beauty, is always in some measure in the eye of the beholder. If he knows something about the field, it is easier to understand; if he knows nothing, it is "unreadable."

Furthermore, Edwards comes to *any* sociological work with the most violent of prejudices against its style and a readiness to attack it on these grounds. Moralizing about sociology in general, he writes: "Half of an ordinary sociology book is dull, stale, and commonplace. Half of the rest is diffuse beyond the point of tediousness." Obviously, in Mr. Edwards' eyes you are damned simply by virtue of being a sociologist. Thus he remarks, "The ineffectiveness of sociologists is a by-word." With such attitudes, I fail to understand why, even if he were competent to review a specialized book such as this, he would *want* to do so. It seems to me, though, that if he did undertake to do such a review he had a responsibility to read the book, not the blurb. He had, I should think, some duty to consider the book's ideas and content, instead of expending his energies biting the hand that prints him.

ALVIN W. GOULDNER

*The University of Buffalo*

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# OFFICIAL REPORTS AND PROCEEDINGS



## REPORT OF THE COMMITTEE ON NOMINATIONS

*To The President of the American Sociological Society:*

As Chairman of the Committee on Nominations, I submit herewith the report of the Committee, giving the results of the election of officers of the American Sociological Society for 1952 and the President-Elect for 1953.

The official ballot for the election was prepared by the Committee on Nominations and mailed by the Executive Officer of the Society to all voting members on March 21, 1951. The nominees for the elective offices were as follows:

### *For President-Elect*

Samuel A. Stouffer, Florian Znaniecki

### *For First Vice-President*

Clifford Kirkpatrick, Richard T. LaPiere

### *For Second Vice-President*

Clyde W. Hart, Joyce O. Hertzler

### *For Committee on Publications*

Jessie Bernard, Marshall B. Clinard, H. Warren Dunham, Alfred R. Lindesmith, Delbert C. Miller, John Useem

### *For Council*

John W. Albig, Harry Alpert, Read Bain, Howard Becker, Leonard Broom, Paul K. Hatt, Thomas C. McCormick, Robert K. Merton, Lowry Nelson, Theodore M. Newcomb, Ira DeA. Reid, Calvin F. Schmid, Raymond F. Sletto, Conrad Taeuber, Logan Wilson, Robert F. Winch

In all, 964 ballots were returned to the Chairman by the stipulated date, April 25th. All were properly identified; hence none was rejected as ineligible. Clement Mihanovich, Richard Dewey, Edward B. Olds, and Robert Habenstein, as tellers, officiated with the Chairman of the Committee in counting the ballots. On the whole, the votes were well distributed among the nominees and in most cases results were fairly close. The following persons, however, were elected by clear majorities:

### *President-Elect*

Samuel A. Stouffer

### *First Vice-President*

Clifford Kirkpatrick

### *Second Vice-President*

Joyce O. Hertzler

### *Committee on Publications* *Term*

Jessie Bernard	3 years
Alfred R. Lindesmith	2 years
John Useem	1 year

### *Council* *Term*

Howard Becker	3 years
Robert K. Merton	3 years
Theodore M. Newcomb	3 years
Conrad Taeuber	3 years
Read Bain	2 years
Ira DeA. Reid	2 years
Lowry Nelson	1 year
Logan Wilson	1 year

This year's Committee on Nominations found its task somewhat complicated by the adoption of the revised Constitution which left several problems of procedure to be worked out. After consultation with some of the officers of the Society, members of the Committee voted to proceed on the assumption that the four persons with unexpired terms on the old Executive Committee will continue as members of the new Council. Of the 16 persons nominated for membership in the Council, it was proposed that the four receiving the highest number of votes serve for 3 years, the next two for two years and the next two for 1 year. (There will be two holdovers for one year and two holdovers for two years.)

Of the ballot received by the Chairman approximately 75 carried write-in votes, but no write-in candidate received as many as 10 votes for a particular office.

In view of the large number of ballots cast and the fact that there were 28 names on each ballot, the task of counting required nearly a full day's work by the four tellers, four clerks, and the Chairman. It is suggested that in future elections the tabulation be entrusted to an outside agency with suitable machines.

Members of the Committee on Nominations were:

Belle Boone Beard  
Ernest W. Burgess  
W. F. Cottrell  
Stuart C. Dodd  
Ernest Greenwood  
Rudolf Heberle  
A. B. Hollingshead  
Henry Meyer

Harry E. Moore  
Harriet R. Mowrer  
Edgar A. Schuler  
William H. Sewell  
Elbridge Sibley  
James W. Woodard

Respectfully submitted,

STUART A. QUEEN

*Chairman, Committee on Nominations*

### AMERICAN PSYCHOLOGICAL ASSOCIATION ANNUAL MEETING

The American Psychological Association will hold its annual meeting in Chicago, Illinois at the Sherman Hotel, August 31 through September 5. Sessions of research reports and symposia have been planned by all divisions of the APA. Sociologists will be primarily interested in the programs of the Division of Personality and Social Psychology (Division 8) and the Society for the Psychological Study of Social Issues (Division 9), outlines of which are given below. Members of the American Sociological Society are cordially invited to attend any sessions in which they are interested.

### DIVISION OF PERSONALITY AND SOCIAL PSYCHOLOGY (DIVISION 8)

#### SESSIONS OF CONTRIBUTED PAPERS

Personality Assessment I. (Kenneth R. Hammond, Chairman)

10:00-11:00 AM, Friday, August 31, Louis XVI Room, Sherman Hotel

Personality Assessment II. (Rex Collier, Chairman)

11:10-12:10 PM, Friday, August 31, Louis XVI Room

Personality Dynamics. (D. W. MacKinnon, Chairman)

10:00-11:00 AM, Sunday, September 2, Ballroom

Personality and Perception. (Launor F. Carter, Chairman)

11:10-12:10 PM, Sunday September 2, Ballroom

Minority Group Problems. (Morton Deutsch, Chairman)

4:00-5:00 PM, Sunday, September 2, Crystal Room

Studies on the Self Concept. (Richard S. Crutchfield, Chairman)

8:50-9:50 AM, Monday, September 3, Bal Tabarin

Interaction of Individual and Social Factors (M. B. Smith, Chairman)

10:00-11:00 AM, Monday, September 3, Bal Tabarin

Studies in Group Dynamics (Robert French, Chairman)

11:10-12:10 PM, Monday, September 3, Bal Tabarin

Studies in Language Behavior (Richard L. Solomon, Chairman)

1:40-2:40 PM, Monday, September 3, Bal Tabarin

Methodology in Mass Communications Research (Carl I. Hovland, Chairman)

2:50-3:50 PM, Monday, September 3, Bal Tabarin

Genesis of Prejudice (Donald T. Campbell, Chairman)

4:00-5:00 PM, Monday, September 3, Bal Tabarin

#### SYMPOSIA

Symposium: Psychological Factors in International Tensions. 1:40-3:50 PM, Friday, August 31, Bal Tabarin

Ross Stagner, Chairman; Daniel J. Levinson, G. M. Gilbert, and Otto Klineberg

Symposium: Difficulties Encountered in Applying Theories to Research in Personality and Social Development of Children. 4:00-6:00 PM, Friday, August 31, Bal Tabarin

(Co-sponsored with Division on Childhood and Adolescence)

Vincent Nowlis, Chairman; Robert G. Barker, Else Frenkel-Brunswik, Merrill Roff, and John Whiting

Symposium: American Culture and American Personality. 1:40-3:50 PM, Saturday, September 1, Bal Tabarin

Abraham H. Maslow, Chairman; Walter Weisskopf, Alfred McClung Lee, Otto Klineberg, Nathan Leites, Martha Wolfenstein

Symposium: Analysis of Language Behavior in Personality and Social Studies. 4:00-6:00 PM, Saturday, September 1, Bal Tabarin

Charles E. Osgood, Chairman; Weston A. Bousfield, Harold Guetzkow, O. Hobart Mowrer, and Ralph K. White

Symposium: Psychology and Linguistics—A Report of the Cornell Conference. 1:40-3:50 PM, Sunday, September 2, Crystal Room

(Co-sponsored with Division of Experimental Psychology)

John B. Carroll, Chairman; Richard L. Solomon, Charles E. Osgood, Thomas A. Sebeck, and J. C. R. Licklider

Symposium: Theoretical Models and Personality Theory. 10:00-12:10 PM, Tuesday, September 4, Ballroom

(Co-sponsored with Division of Clinical and Abnormal Psychology)

George S. Klein, Chairman; Andras Angyal, Jerome S. Bruner, Ward C. Halstead, D. O. Hebb, Donald MacKinnon, James G. Miller, Neal Miller, and David Rapaport

Symposium: Cooperative Research Programs Involving Sociologists and Psychologists. 3:30 PM, Wednesday, September 5, Louis XVI Room

(Co-sponsored with American Sociological Society)

Samuel Stouffer, Chairman

## THE SOCIETY FOR THE PSYCHOLOGICAL STUDY OF SOCIAL ISSUES (DIVISION 9)

### SYMPOSIA

Symposium: Values and Social Class. 2:10-3:40 PM, Tuesday, September 4, Crystal Room

S. M. Miller, Chairman; Richard Centers, Daniel Katz, and Alfred McClung Lee

Symposium: Applications of Social Science to the Problems of Underdeveloped Areas. 2:50-4:20 PM, Tuesday, September 4, Primrose Room

S. P. Hayes, Jr., Chairman

Symposium: Methods of Communicating Psychological Knowledge to Relevant Publics. 4:00-5:30 PM, Tuesday, September 4, Bal Tabarin

Nathan Maccoby, Chairman; Fred Palmer, Stuart Chase, Alton Blakeslee

Symposium: Communication, Public Opinion, and International Relations. 10:00 AM, Wednesday, September 5, Sheraton Hotel

(Co-sponsored with American Sociological Society)

Bernard Berelson, Chairman; Hans Speier, Charles Glock, Paul Lazarsfeld, Alex Inkeles, Homer F. Geiger, and Douglas Waples

Symposium: Criteria for Useful Labor Management Research. 1:40-3:40 PM, Wednesday, September 5, Bal Tabarin

(Co-sponsored with the Industrial Relations Research Association)

Arthur Kornhauser, Chairman; Reinhard Bendix, Arthur Ross, Arnold Rose, and Ross Stagner

## NEWS AND ANNOUNCEMENTS



### **International Conference of Social Work.**

The sixth world-wide meeting of the I.C.S.W. will be held in Madras, India in December 1952, it has just been announced by George E. Haynes, President of the organization. The theme of the Conference will be "The Role of Social Service in Raising Standards of Living." The program will stress the social welfare needs and problems of under-developed countries, with special emphasis on the Far Eastern region.

Since its beginning the International Conference has met in Paris (1928 and 1950), Frankfurt-on-Main (1932), London (1936) and Atlantic City (1948). The 1952 Conference will be the first to be held in the Far East.

In an effort to make it practicable for as many Americans as possible to attend the 1952 Conference in India, the United States Committee is working on plans for group travel to and from India at reduced fares. Persons interested in securing additional information can do so by writing to the headquarters of the I.C.S.W., 22 West Gay Street, Columbus 15, Ohio.

**The Oslo Institute of Social Research** is planning a Conference and Seminar during 1951 focused on the problem of comparative social research in Northern European countries. The conference will begin in September 1951, and last for approximately three months. Participants from a number of European countries will be invited. The members of the conference will plan in detail the first of a series of comparative projects to be undertaken in 1952. The first specific project chosen will be decided upon by the participants themselves during the course of their meetings. However, any project chosen will be limited to one of importance within the general area of international tensions.

Organizational plans are now being worked out by a special committee working under the chairmanship of Dr. Herbert Hyman, Visiting Professor of Social Psychology for 1950 under the Fulbright Act. During the course of the conference the participants will have the opportunity to learn about recent methodological developments in social research from leading social scientists. Dr. Daniel Katz of the University of Michigan, who is expected to join the Oslo Institute as Visiting Professor for 1951 will participate in this phase of the project. This phase will help provide a common point of view and ensure the high degree of uniformity necessary for effective cross-national research.

Since such a conference would provide unusual opportunities for training and cooperation with

fellow scientists in other countries and would lay the groundwork for the first large-scale international research program on social problems of great importance, it has already met with considerable interest. The International Sociological Association has sponsored the seminar.

Participants will receive their travel expenses and a stipend at the salary level of a Norwegian Professor for the entire period of the conference. Only one social scientist from each country other than Norway, can be included. Other requirements are that the participant be fluent in English, the language in which the conference will be conducted; be at an advanced professional level of training; be not too advanced in age; be in the field of Social Psychology or Sociology; and be oriented in the direction of empirical social research.

Since the final selection of candidates has not yet been made, applications will still be considered. Address all such applications or any other inquiries to: Erik Rinde, Chairman, Oslo Institute of Social Research, Grev Wedels pl. 4, Oslo, Norway.

**American Council on Education.** Limited numbers of Bulletin No. 171, "Recommendations of the National Student Conference on Human Relations in Higher Education," may be procured without cost upon request to the Committee on Discriminations in Higher Education, 1785 Massachusetts Ave., N.W., Washington 6, D.C. The complete report should be available in the fall.

**Anti-Defamation League**, through its national and regional offices, is participating in Summer Workshops in democratic living in 28 colleges and universities throughout the country. The Workshops are directing their attention primarily to the following areas: Materials and Techniques in Teaching Human Relations, Practices in Intergroup Education, School and Community Relations, and New Horizons for the Public Schools.

**Committee for the Scientific Study of Religion.** The Spring meeting of the Committee for the Social Scientific Study of Religion was held at Harvard University on April 21, when it was decided to change the name to the Committee for the Scientific Study of Religion. Professor Talcott Parsons of the Department of Social Relations at Harvard was elected the new Chairman of the Committee. Other officers are Professor Prentiss Pemberton of Andover-Newton Seminary, Vice Chairman, and Professor J. Paul Williams, Mount Holyoke



College, Secretary-Treasurer. The next meeting is to be held at Harvard on November 3rd. Those interested should write the Secretary-Treasurer. Social scientists who may have research of an empirical nature in the field of religion to report at the next meeting should write Dr. Parsons before October 1st about the possibility of a place on the program.

**Cooperative Bureau for Teachers.** Dr. Rosemary Park, President of Connecticut College and Chairman of the Board of the Cooperative Bureau for Teachers, 1776 Broadway, New York City, announces the appointment of Dr. James L. Whitehead as Director of the College Department of the Bureau. A non-profit member organization devoted to the improvement of standards in the teaching profession, the Bureau is governed by a Board of college and school administrators and teachers. Founded in 1924, the Bureau has been concerned with placement of teachers and administrators, and with research on problems involving personnel for elementary and secondary schools. In 1947, at the request of a group of liberal arts colleges, a placement service for college teachers was established on an experimental basis. A series of informal discussions with administrators and department heads, and a careful review of the doctoral programs for prospective college teachers, provided a starting point. After three years, this service has been organized on a permanent and fully professional basis under Dr. Whitehead's direction. Each member college receives as complete a placement service as the Department can give, regardless of the number of vacancies. In addition to the member colleges, approximately seventy-five institutions list their vacancies.

The College Department will be helpful, not only to faculty members seeking new positions, but also to students entering the field of college teaching. It will serve as a much-needed clearing house through which good teachers may find challenging opportunities, receive recommendations to posts for which they are well suited, and find help both in meeting crises in employment and developing plans for further study.

In line with the Bureau's traditional policy the Department is cooperative in character. Each member college appoints a representative to the Governing Board for the formulation of policies. Special meetings of the representatives are held for discussion of common problems. As with schools, the placement service for college teachers and the study of personnel problems is under the guidance of a committee of the Board. Dr. Louis T. Benezet, President of Allegheny College, represents the colleges and their concerns on the Executive Committee.

**Eastern Sociological Society.** A new procedure adopted at the New Haven meetings provides for the election of officers by a mail ballot. The Nominating Committee will submit its list of

nominees. In addition, members are reminded that any ten members of the Society may submit nominations to the Secretary, Professor Bernhard J. Stern, not later than January 1st for inclusion on the ballot.

The tentative program of the 1952 annual meeting, to be held at Haverford College, Haverford, Pennsylvania, on April 5-6, is as follows:

Saturday, April 5

10-12 A.M. Contributed Research Papers

1:30-4:30 P.M.

(1) Interrelations of Sociology and Economics

(2) Buyers and Sellers in the Research Market

4:30 P.M. Annual Business Meeting

7:00 P.M. Annual Dinner

(1) Presidential Address

(2) Invited Speaker

Sunday, April 6

10-12 A.M.

(1) Recent Developments in American Social Structure

(2) Sociological Implications of Volume II of the Kinsey Report

1:30-3:30

(1) Recent Advances in Methodology

The president of the Society, Professor Jessie Bernard, Pennsylvania State College, State College, Pennsylvania, will be pleased to receive suggestions for speakers from members of the Society. All other queries relative to the meeting may be addressed to the secretary, Dr. Bernhard J. Stern, Columbia University, New York 27.

**Ohio Valley Sociological Society** held its thirteenth annual meeting April 27 and 28 at Indiana University, Bloomington, Indiana. Approximately 200 were in attendance. World crisis was a major motif of the meeting, three sections dealing respectively with World Crisis as a Challenge to Sociology, Prospects for Sociological Achievement in the World Crisis, and War as a Social Institution. Other sections were devoted to papers on Social Psychology, Converging Research, Family, Racial and Ethnic Groups, and Social Organization and Process.

At the concluding session of the meeting, Julian Huxley, Patten Lecturer at Indiana University, spoke briefly on UNESCO's projected cultural history of mankind.

H. Warren Dunham presided at the annual dinner meeting, and J. Milton Yinger gave the presidential address: "Toward a Theory of Prejudice."

In a general session, resolutions of expression of sympathy were extended to the families of the late Edwin H. Sutherland and Luther L. Bernard.

The next annual meeting will be held at Michigan State College, East Lansing, on April 25-26, 1952.

Officers for 1951-52 are: President, William Form, Michigan State College; Vice-President, Clifford Kirkpatrick, Indiana University; Secretary-Treasurer, Stuart Adams, College of Wooster. Brewton Berry, Ohio State University, continues as Editor of the *Ohio Valley Sociologist*.

**The Rural Sociological Society.** The 1951 annual meeting will be held at the University of Wisconsin, Madison, Wisconsin, on September 2-4, followed by joint sessions with the American Sociological Society at the Sheraton Hotel, Chicago, Illinois, on September 5-7. At the Wisconsin meeting, sessions will be devoted to the following topics: research methods, studies in the adoption of farm and homemaking practices, social stratification, community studies, potential contributions of sociological research to extension programs, acculturation of ethnic groups in American society, studies in social change, studies in the development of consensus, and population. The presidential address of Robert A. Polson, on "Sociological Training for Professional People from Other Cultures," will be given at the annual dinner held jointly with the American Sociological Society on September 6.

**Upstate New York Sociological Society** held its spring meeting in conjunction with the Tri-State Council on Family Relations on May 11 and 12 at Syracuse University. The theme of the conference centered about the teaching of Marriage and Family courses. The following topics were discussed:

1. Underlying Philosophy of Marriage Courses in Our Colleges

Discussants: Professor Lemo Rockwood, Cornell; Professor Don Taylor, Colgate; and Professor Seward Salisbury of State University of New York, Teachers College, Oswego.

2. Scope, Content, and Implementation of the Functional Marriage Course for Undergraduates

Chairman: Professor Eugene Link, State University of New York, Teachers College, New Paltz.

3. Scope, Content and Approach of Sociology of the Family

Chairman: Professor Wendell Bash, Colgate  
Panel: Professor David Hatch, Syracuse; Professor Gordon Streib, Cornell; and Professor Paul Wheeler, State University of New York, Teachers College, Albany.

The following research papers were presented: "Local and Absentee Concentration of Economic Power: Theoretical and Empirical Differences," by Mr. Irving Fowler, Cornell; and "Intergroup Attitudes and Leadership Patterns in a Bi-racial Section of Albany," by Professor Ted Standing, State University of New York, Teachers College, Albany.

The dinner address was given by Dr. Donald Snygg of State University of New York, Teachers College, Oswego, on "Some Implications of Phenomenological Psychology for Group Behavior."

Professor Barbara Griggs is the Chairman of the New York area of the Tri-State Council. Professor Seward Salisbury is Chairman and Professor Vladimir de Lissovoy is Secretary-Treasurer of the Upstate Sociological Society.

**Boston University.** Announcement is made of a new program in Criminology and the Correctional Treatment of Offenders to begin with the academic year 1951-52. Planned and supervised by Professor Albert Morris, chairman of the Department of Sociology and Anthropology, the program is intended to provide pre-professional training for those considering a career in crime prevention and corrections, to supplement training programs for those already in the field, and to contribute to a better understanding of the nature and treatment of delinquents and criminals by the general public and especially on the part of workers in allied fields.

**City College of New York.** John Collier has been dividing his energies between the non-developed areas of the world and the affairs of the American Indians, being frequently called upon by various government agencies to advise on social planning and engineering.

Harry M. Shulman is directing a field-laboratory program combining service and student-training.

Adolph S. Tomars was visiting professor at Hofstra College last summer.

Warren Brown has completed directing his third field study of small industrial communities, including Willimantic, Conn., and Coatesville and Bethlehem, Pa. He is chairman of the research sub-committee of the New York Welfare Council's Puerto Rican Committee.

Alfred Parsell was chairman of the session on the Migrant Worker in the Community at the National Conference of Social Work in May.

**Columbia University, Bureau of Applied Social Research.** Professor Eugene L. Hartley is on leave from the City College of the College of the City of New York, and is serving as European Field Director for the Air Force Project of the Bureau of Applied Social Research of Columbia University.

**Dartmouth College.** The following men are at present members of the Department of Sociology: Professors Rees Bowen, Michael Choukas, H. Wentworth Eldredge, Ralph Holben, Robert A. McKennan, Francis Merrill, George F. Theriault; Assistant Professor Elmer Harp, Jr.; and Mr. Robert Gutman, Instructor. Professor Michael Choukas is chairman.

Three former members of the Department hold the rank of Professor Emeritus: John Mecklin, Erville B. Woods, and McQuilkin DeGrange. Pro-

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essor DeGrange retired from his teaching duties and was appointed Professor Emeritus in June, 1950.

George Theriault has been awarded the degree of Ph.D. in Sociology by the Department of Social Relations at Harvard; he was recently elevated to the rank of Professor. Professor Theriault has been appointed a member of the Citizens Council Subcommittee of the New Hampshire Conference of Social Welfare and Citizens Councils.

Elmer Harp, Jr., Curator of Anthropology in the Dartmouth College Museum, has been appointed Assistant Professor of Sociology. He has been awarded a grant-in-aid by the Arctic Institute of North America to continue his investigation of the Boethuck Indians and the Cape Dorset Eskimos.

The Department issued in May, 1951 the second annual *Sociology Bulletin*. The *Bulletin* describes the activity of the Department staff and undergraduate majors during the past year. It is circulated to alumni Sociology majors of the College.

The Department of Sociology, in collaboration with the Department of Psychology, has inaugurated for advanced students a seminar concerned with problems common to sociology and psychology. Mr. Robert Gutman of the Department of Sociology and Professor Albert Hastorf of the Department of Psychology led the seminar during the past academic year.

**Haggard Clinic**, Nashville, Tennessee. McNairy M. Crutchfield, psychologist, an associate member of the American Sociological Society, died late in April this year.

**Morehouse College.** The Sixth Annual Institute on Building for Successful Marriage and Family Living was conducted by the Department of Sociology at Morehouse College with the assistance of the Planned Parenthood Federation of America, Inc., March 14-16, 1951. The General Theme was "A Warrant of Security—The Family." Participants on the Institute program included Mr. William M. Cooper, Registrar, Hampton Institute, Hampton, Virginia, and Secretary-Treasurer of the National Conference on Adult Education and the Negro; Dr. W. Gordon Ross, Professor of Philosophy and Religion, Berea College, Berea, Kentucky; Miss Mary E. Langford, Field Consultant, Planned Parenthood, Federation of America, Inc., of New York City; Dr. Richard I. Porter, Associate Pastor, First Community Church, Columbus, Ohio; and Mrs. Frances Logan, Friends Neighborhood Guild, Philadelphia, Pennsylvania, and Cooperating Supervisor of Field Work, Atlanta University School of Social Work.

This Annual Institute is under the direction of Professor Walter R. Chivers, Chairman, Department of Sociology, Morehouse College, Atlanta, Georgia. Mrs. C. Gartrell Chivers, Instructor in Sociology serves as Associate Director. Mr. Chivers has lectured in the field of marriage and family problems in thirteen states and the District of Columbia. He has also helped to initiate Marriage and Family

Institutes in colleges, high schools and churches in the South and has participated in Institutes in the capacity of Consultant at Howard University, South Carolina State College, Florida A. and M. College, Savannah State College, Albany State College, The First Baptist Church at LaGrange, Georgia, Talladega College, Tillotson College, Virginia State College, Miles College, Alcorn College and Prairie View College. Mrs. Chivers has worked with him at Miles College, Talladega College, and the Hutto High School in Bainbridge, Georgia.

**Northwestern University.** Among the academic conferences commemorating Northwestern's Centennial observance throughout 1951 were those on Problems Related to Human Behavior, held on May 17-19, and Problems of an Aging Population held on June 7-8. Sociologists who participated included Leonard S. Cottrell, Jr. of Cornell University, Philip Hauser of the University of Chicago, Rupert B. Vance of the University of North Carolina, and Clyde Hart of the National Opinion Research Center.

**Princeton University.** Wilbert E. Moore has been promoted to the rank of professor and Marion J. Levy, Jr., to the rank of associate professor. Melvin M. Tumin has been named to a special preceptorship, which provides that for one year of a three-year term he will be on salaried leave for research, and that each year he will have a small drawing account for research expenses.

Duncan MacRae has resigned to accept a research position at Harvard. George W. Barclay has been appointed instructor in sociology and research assistant of the Office of Population Research. Harold Garfinkel, instructor in sociology, will devote half-time during 1951-52 to the Organizational Behavior Project. This Project is an interdisciplinary research planning operation, which is the initial program at Princeton for use of the Ford Foundation grant for the behavioral sciences. The planning team comprises men from political science, economics, psychology, sociology, history, and mathematics. Professor Moore serves as director. Single copies of the first progress report are available on request as long as a limited supply lasts.

Don J. Hager, assistant professor, is spending the summer at the University of Michigan to prepare an initial design for long-term research in social biology. Gerald W. Breese, assistant professor and director of the Bureau of Urban Research, starts this summer on a two-year project cooperatively with the University of Pennsylvania, studying urbanization in the Morrisville, Pennsylvania area, site of the new United States Steel plant.

**Saint Louis University.** An institute on the Behavior of Children in Institutions was conducted during the 1951 summer session at the School of Social Service by Sister Agnita Miriam of the National Catholic School of Social Service of Catholic University.



Miss Lucile Healy, assistant professor of social work, was recently elected chairman of the Missouri-Kansas division of the American Association of Medical Social Workers.

**Stanford University.** Robert C. Stone resigned from the staff of the Department of Sociology and Anthropology to accept a position at the Urban Life Research Institute of Tulane University.

Richard J. Hill and Henry Zentner were appointed to give the courses that Mr. Stone was scheduled to teach during the spring quarter. Gregory Bateson, who is currently doing research at the Veterans Administration Hospital, Palo Alto, and serving as a consultant at the Langley Porter Psychiatric Clinic, San Francisco, offered a seminar under the auspices of the department on "Communication and Cultural Transmission" during the spring quarter.

Felix M. Keesing, currently on leave for a year of research and travel in the South Pacific, is expected to return to the university this summer and will resume his duties as Executive Head of the joint department in the fall.

**United Nations.** Robert C. Jones is spending the summer months in Ecuador as a consultant on community development for the Technical Assistance Administration of the United Nations.

**University of Buffalo.** Nathaniel Cantor, Chairman of the Department of Sociology and Anthropology, has accepted an invitation from Columbia University to serve as Professor of Sociology for the academic year 1951-1952. This appointment is one of the three made under the Carnegie Grant in General Education.

**University of Miami.** A summer workshop in Intergroup Education was held June 15-July 3, under the direction of M. A. F. Ritchie, chairman of the Department of Human Relations. Among the out-of-state participants were Dr. Dan W. Dodson of New York University and Dr. Herbert L. Seasmans of the National Conference of Christians and Jews.

**University of North Carolina.** Two additions have been made to the anthropological staff. Dr. Frank LeBar has been named assistant professor, and Dr. John J. Honigman associate professor. Both have also been appointed research associates in the Institute for Research in Social Science. Dr. LeBar, in addition to offering courses on Oceania, primitive technology, and Southeast Asia, is serving as field director of a cultural study of the Veterans Administration psychiatric hospital at Roanoke, Virginia. Dr. Honigman is offering courses in general anthropology, primitive religion, and field methods,

and will devote about half his time to collaboration in a research project concerned with the cultural organization of U. S. Air Force bases. The staff in anthropology at Chapel Hill now includes John Gillin, professor and research professor; Guy B. Johnson, professor and research professor of anthropology and sociology; John J. Honigman, associate professor and research associate; Frank LeBar, assistant professor and research associate; Joffre Coe, instructor and director of the laboratory of anthropology and archaeology.

**University of Pittsburgh.** The pre-enrollment for the Department of Sociology indicates an increase in advanced students for the coming sessions this summer and next fall.

In addition to the regular members of the staff—Dr. H. A. Phelps, Dr. D. B. Rogers, Dr. V. C. Wright, Dr. Howard Rowland, Dr. David Henderson and Dr. Lawrence Hugo—visiting lecturers will include Professor George P. Murdock of Yale University and Professor Alfred R. Lindsmith of Indiana University.

Dr. M. C. Elmer will be in Venezuela studying various sociological aspects of life and living conditions in that country.

**University of Southern California.** The Salzburg Seminar in Human Relations has invited Harvey J. Locke to be visiting professor for the summer session of 1951. In addition to Uppsala University, where he is visiting professor in the Sociology Institute, he has lectured at the University of Stockholm, Lund University, the University of Copenhagen, and the University of Oslo.

**Washington University.** Professor Walter B. Bodenhafer who has been a member of this department since 1920 became Professor Emeritus at the end of the 1950-51 academic year. He will, however, continue to teach during the fall semester.

**Yale University.** Announcement is made of a new Master of Arts in Teaching program. Its fundamental purpose is to attract outstanding personalities into secondary-school teaching and by giving them a sound liberal training in the subject or subjects they propose to teach, as well as in the professional courses necessary for state certification, to improve the quality of teaching.

The venture is sponsored jointly by the Department of Education, which will be chiefly responsible for the professional aspects of the program, and by the academic departments, which will have the responsibility for making sure that the students possess an adequate knowledge of the subjects they propose to teach. Director of the Program is Dr. Theodore Andersson, Hall of Graduate Studies, Yale University, New Haven, Conn.

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## HERBERT ADOLPHUS MILLER, 1875-1951

In Black Mountain, North Carolina, on May 7, the eminent sociologist, Herbert Adolphus Miller, died after a long illness. Thus death brought to an end a distinguished triumvirate of pioneer scholars whose field was racial and minority groups—W. I. Thomas, Robert E. Park, and Herbert A. Miller.

Herbert Adolphus Miller was born at Tuftonboro, New Hampshire June 5, 1875. His mother was of Welsh origin, his father a Swede. He graduated from Dartmouth College in 1899 with honors in philosophy. While at Dartmouth he had a course in ethnology and sociology with David Collins Wells, one of the first teachers of sociology in this country. The course in ethnology strongly emphasized the classification of races into superior and inferior. Upon graduation Miller accepted a position at Fisk University where he was engaged to teach Greek and athletics. He stated in later life that while at Fisk he became convinced that Keane's *Ethnology* was wrong. Years later he became a trustee of Fisk University.

After three years at Fisk, Professor Miller went to Harvard to study philosophy with the idea that ethics and psychology formed the proper approach to a scientific study of the problem of race. While at Harvard he studied with Josiah Royce and William James. At the suggestion of Professor R. M. Yerkes, and with money raised by William James, he devised a series of intelligence tests and gave them to Negro and white students in the South. The white students were in mountain schools that represented a more comparable environment with that of the Negroes. He also gave the tests to several hundred Indians at Hampton Institute and Carlisle Indian School.

After receiving his Doctor's degree at Harvard in 1905, the title of his dissertation being "Psycho-Physics and the Race Problem," Miller went to Olivet College in Michigan where he taught philosophy and sociology for ten years. In 1911 he first met W. I. Thomas. He felt that Thomas' point of view regarding race represented his own and, accordingly, went to the University of Chicago for the summer and autumn quarters. It was at this time that he became acquainted with the Bohemian, or Czech, immigrants whose background and national aspirations intrigued him throughout

the remainder of his professional life. During the summer of 1912 he visited Bohemia with a large delegation of Sokol gymnasts. He immediately met Professor John Masaryk who taught sociology at Charles University in Prague. Professor Miller also visited Poland, Russia, and Finland. As early as 1912 he prepared a paper for the American Sociological Society in which he prophesied that the growing nationalism of dominated peoples throughout Europe would make war inevitable.

In 1914 Professor Miller went to Oberlin College to teach sociology. He remained there until 1924 when he accepted a position at Ohio State University.

In 1915 he was asked by the Russell Sage Foundation, then making a survey of the schools of Cleveland, Ohio, to make a report on *The School and the Immigrant*. This was published in 1916.

In 1917 he was asked by the Carnegie Corporation to assist in making a study of the methods of teaching Americanization. Others who worked on this study were Thomas and Park. It resulted in the publication of the book *Old World Traits Transplanted* bearing the names of Park & Miller.

During World War I Professor Miller was called to Camp Sherman, Chillicothe, Ohio, to advise the commanding general concerning the disposition of thousands of "alien enemies," members of nationality groups whose countries were at war with the United States. Due to his interpretive analysis before the officers of this cantonment, the government initiated a policy of recognizing the war aims of these dominated peoples.

During the war also he was the guiding American in founding the Mid-European Union and in engineering the Czechoslovak Declaration of Independence at Philadelphia in 1918. He worked with both President Masaryk and Ignace Paderewski, first president of Poland.

In 1924 Professor Miller published his best-known work *Races, Nations, and Classes*, in which he developed his concepts of vertical and horizontal groupings and the oppression psychosis as applied to minority groups.

During his sojourn at Ohio State University (1924-1931) he went to India to observe Gandhi's passive resistance campaign. At one

of the open meetings in Bombay Professor Miller made a short speech that aroused the trustees of Ohio State University to a point that they refused to renew his contract. Another complaint of the trustees was that Professor Miller did not condemn students of his Race Problems course for dancing with colored students while on a visit to Wilberforce University in Ohio. The American Association of University Professors made an investigation and subsequently exonerated Professor Miller, and as a result Ohio State University was for several years placed on that organization's list of censured universities.

The next two years after leaving Ohio State University Professor Miller traveled, lectured, and wrote *The Beginnings of Tomorrow*. This work was published in 1933. In that year he was offered a position at Bryn Mawr College where he remained until the age of retirement in 1940. Subsequently he taught various semesters at Temple University, Pennsylvania State College, and Beloit College. In January 1943 he went to Black Mountain College where he remained until September 1947; thus Professor Miller rounded out seven years after formal retirement.

During the summers of 1940-1943 Professor Miller directed the "American Seminar for Refugee Scholars" under the auspices of the American Friends Service Committee. Hundreds of newly arrived European scholars received their first real impressions of democracy, as well as the fundamentals of English as it is spoken here, at these camps in New Hampshire. Professor Miller often remarked that this period was one of the most interesting of his entire life.

Herbert Adolphus Miller was a kindly man. He loved teaching as few men do. He found good in everyone. He inspired not only good scholars but also hundreds of average students who literally found themselves in the study of sociology as interpreted by him. He was a real friend of members of all minority groups. At one time he taught a semester at Yenching University in Peking and had a host of friends among Chinese intellectuals as well as among the common people of that great country. Not a few of his students later traveled across the world to visit him in his home.

It would be difficult to appraise Professor Miller's many contributions to the field of sociology. He was a teacher, a social philosopher, and a crusader in the area of international co-operation. Because of his contributions in this latter area he was the recipient of awards from President Eduard Benes of Czechoslovakia and Syngman Rhee of Korea.

His great contribution during the period just prior to the armistice in World War I represents something unique. He worked close to diplomats and statesmen and even discussed the policy of self-determination of dominated peoples with President Woodrow Wilson. His unpublished memoirs of that period make fascinating reading, especially in the light of the tragic era that led to World War II and the travail of the United Nations.

He is survived by his wife, Bessie Cravath Miller, who resides in Black Mountain, North Carolina.

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## BOOK REVIEWS



*Suicide: A Study in Sociology.* By EMILE DURKHEIM (Translated by JOHN A. SPAULDING and GEORGE SIMPSON; Edited with an Introduction by GEORGE SIMPSON). Glencoe, Ill.: The Free Press, 1951. 405 pp. \$5.00.

*The Rules of Sociological Method* (Eighth Edition). By EMILE DURKHEIM (Translated by SARAH A. SOLOVAY and JOHN H. MUELLER; and edited by GEORGE E. G. CATLIN). Glencoe, Ill.: The Free Press, 1950. lx, 146 pp. \$2.50.

In view of the growing interest in the United States in European sociological theory, we are, indeed, deeply indebted to the editors, translators, and publishers who have rendered the invaluable service of making available to the English-speaking peoples these classics of French sociology.

*Traduttore, traditore* the Italians say; and since translators must be traitors we can judge them only by the gravity of their treason. Translators have a difficult course to steer between the Scylla of literal but incomprehensible rendition and the Charybdis of free but unfaithful translation. John Spaulding and George Simpson have produced a fair translation, but have directed their course Scyllaward, thus tending to err on the side of excessive literalness. This results in unpolished and sometimes incomprehensible English sentences. This becomes evident when one compares the Spaulding-Simpson version with the fragments of *Le Suicide* which were translated for Sorokin, Zimmerman and Galpin's *Source Book in Rural Sociology* (vol. III). Here are some sample comparisons:

S-S version: "confirm this first conclusion" (p. 152)

S-Z-G version: "confirm this tentative conclusion" (p. 190)

(In this instance, the French "*premier*," while literally "first" is better rendered as "tentative.")

S-S version: "The aptitude of Jews for suicide" (p. 155)

S-Z-G version: "The tendency of Jews to commit suicide" (p. 190)

S-S version: "Switzerland forms an interesting study from this same point of view. For

as both French and German populations exist there, the influence of the confession is observable separately on each race" (p. 154).

S-Z-G version: "Switzerland offers a field for interesting study on this point, for one finds there both French and German inhabitants and may observe the influence of religion upon each race separately" (p. 190).

Other examples of literalness making for bad English are: "The famous crash is unforgotten which took place on the Paris Bourse" (p. 241); "suicides in a million" (Table, p. 262) for "suicides per million"; "state of de-regulation" (p. 253). There is also a ready recourse to visual analogues of the French word ("*inexactitudes*" for "inaccuracies", for example). More serious is a sentence such as "Suicide is like mental alienation" (p. 278). Here the translators have ignored the "*en*" in "*il en est*," thus failing to indicate that the meaning is that suicide is *in this regard* like mental alienation.

Sarah Solovay and John Mueller, on the other hand, have directed their course Charybdisward and have given to their translation an unusually fluent and readable style. To achieve this, however, they have had to edit the original quite liberally and as a result not only is a good deal of Durkheim's own characteristic style lost, but in many places the author's thoughts are betrayed. To illustrate at random: the phrase *sui generis*, so distinctively Durkheimian, is mysteriously eschewed; "*la conscience tant individuelle que sociale*" becomes inexplicably "the individual consciousness" (p. xli); methodical sociology (*sociologie methodique*) is rendered as "methodological sociology" (p. xlii); and the results of our practices (*resultats de notre pratique*) becomes "results of our work in applied sociology" (p. lx); an unnecessary and misleading "therefore" is inserted on p. xlix, rendering Durkheim's thought a *non sequitur*; whereas in the French, Durkheim is ready to accept a criticism, in the English version he becomes more reluctant and condescends only to discuss it (p. liii); at one point the phrase "spontaneous generation" is gratuitously attributed to him (p. xlvii); etc.



*Suicide* undoubtedly reveals Durkheim at his best, and, at the same time, points up his weaknesses. Here we have a good example of his astuteness of sociological analysis, his genius for bringing individual facts into relation with one another in terms of a meaningful conceptual framework, his concern with philosophical implications and practical import as well as theoretical generalizations, his zeal in attacking social phenomena in terms of their most objective and impersonal manifestations (in this case, suicide rates), his dogged persistence in revealing the nature *sui generis* of social phenomena, and his dramatic demonstration that even the apparently highly personal and individual act of self-destruction is explicable only in terms of social processes and social structures. On the other hand, we also see in *Suicide* the extravagant and cavalier manner in which Durkheim used the method of elimination, his intransigence vis-à-vis conflicting viewpoints, the excesses and ambiguities resulting from his polemical style, his sophistry in substituting conjecture for empirical fact, the ineptness of phraseology stemming from his belligerent defense of the reality of social facts, and his uncritical eagerness to resort to crude rates when they helped support his contentions. Despite these faults, none can gainsay the brilliance and originality of this sociological classic.

Students of suicide can no longer rely on Durkheim's work alone. Rather it should be treated as the first of a trilogy, the other two studies with which it is intimately connected being Halbwachs' *Les Causes du Suicide* and Bayet's *Le Suicide et la Morale*. Halbwachs' study is a necessary complement to Durkheim's. It introduces more sophisticated statistical techniques and demonstrates that in several important instances the statistical evidence may lend support to explanations which Durkheim either dismissed or ignored. Marcel Mauss properly calls Halbwachs' book the "indispensable corrective" to Durkheim and warns that it would be unwise, unscientific, and absurd to use Durkheim's *Suicide* without constant reference to Halbwachs. Bayet places the study of suicide on a sounder historical basis and in broader perspective. By introducing attitudinal data derived from laws, literature, newspapers and other documentary sources, he liberates suicide studies from the rigid strait-jacket into which Durkheim, by virtue of an excessive zeal for objectivity, seemed ready to confine them.

Durkheim's rules of method relate to the logic of sociological investigation and the American

researcher who turns to them for guidance in preparing a questionnaire or conducting an interview will be disappointed. But he will not find reading the volume unprofitable. For Durkheim's particular talent in raising fundamental questions and starting suggestive trains of thought will evoke in him a consciousness of the logical and philosophical principles upon which his research procedures rest, a consciousness that is unfortunately lacking among many present-day practitioners.

A distinguished Durkheimian has remarked, "Of all Durkheim's works I like *Les Règles* least." One can readily understand this appreciation, for a small volume describing, all too generally, the working methodological hypotheses of a practicing sociologist is bound, when considered apart from the studies these principles are intended to guide, to appear barren and abstract, even futile. What can be done, for example, with Durkheim's morphological scheme of horde, simple polysegmental society, polysegmental society simply compounded, and polysegmental society doubly compounded? This Spencerian and utterly bewildering schema appears to the contemporary student as an amazing, perhaps ingenious, but surely unusable curiosity. Yet the doctrine that there are social types, that their establishment is a *conditio sine qua non* of comparative sociology inspired in the *Année Sociologique* group a meticulousness in examining ethnographic materials, a cautiousness in utilizing data indiscriminately picked from all societies, and an eschewing of the method of random illustrations from groups everywhere, at all stages of their evolution. These eminently desirable traits of work have earned for this group the admiration of the most exacting of social scientists. Likewise, it is difficult to extract much solid substance from Durkheim's chapter on the distinction between the normal and the pathological, but the efforts to establish a scientific definition of normality have led to an appreciation of the great insight and understanding that come from viewing phenomena such as crime, prostitution, and poverty, not morally and denunciatorily, but scientifically, as the necessary consequences of natural conditions. A veritable revolution has been wrought in criminology, for example, by just such a change in approach. And modern writers, incidentally, have improved but little on Durkheim's theory of crime.

In spite of certain positivistic excesses in the direction of an under-estimation of the role of "prenotions" in all scientific thinking, and of a

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much too rigid conception of objectivity, Durkheim's *Rules* has the enduring quality of all works that deal with first principles zealously, profoundly, scholarly. Surely a methodological treatise that can inspire, among other works, such sociological masterpieces as *Le Suicide*, Mauss' *Essai sur le don*, Granet's *Danses et Legendes de la Chine*, and Levy-Bruhl's *La Morale et la science des moeurs* is of no mean merit.

Professor Catlin's introduction to the translation is a sympathetic treatment of Durkheim which recognizes the French theorist's mastery of method and calls attention, in its conclusion, to the political implications of the Durkheimian view of society: "If, with one hand, Durkheim as sociologist has built the structure of popular dictatorship, with the other he has indicated to us the deeper volcanic forces that must destroy in time, by a certain law of human seismology, every tyranny that does violence to our substantial natures."

In contrast to this sympathetic approach, the Introduction by Dr. Simpson seeks to substitute for Durkheim's strictly sociological position on suicide a more psychoanalytically oriented viewpoint. Simpson's basic underlying hypothesis is that "suicidal behavior is a combination of psycho-instinctual impulse and social precipitation" (p. 26). This relegation of social factors to a mere precipitating role is a fundamental negation of the Durkheimian thesis. Durkheim had contended that it was not possible to investigate suicide as a social phenomenon by seeking to establish types of individual behavior in suicide. This contention is condescendingly dismissed by Simpson with a simple "we now know better."

Simpson's Introduction is a loosely written, rambling essay. It begins with a useful, succinct summary of Durkheim's book, wanders off into a discussion of the contributions of "psycho-analytic psychiatry" to an understanding of suicide, continues with some suggested hypotheses for research, and concludes with some philosophical musings about "the wild and dark furies of irrationality to which human beings are heir" and a plea to fight that irrationality by incorporating the findings of science and human reason "into the social structure and the functioning of the individual in that structure." It contains a number of irresponsible statements and its scholarship is severely limited by an almost exclusive recourse to works available in English. One example is the contention that the chief advances in our knowledge of suicide, since

the publication of Durkheim's study, have come from "actuarial statistics and psychoanalytic psychiatry" (p. 17). This ignores the valuable contributions of Halbwachs, Bayet, Ferri and many others. The discussion of the relation of sociological to psychiatric studies of suicide makes no mention whatsoever of the psychiatric rebuttal to Durkheim by Dr. F.-A. Delmas, the efforts at reconciliation of the two viewpoints by Charles Blondel, or the very suggestive data presented by Max Bonnafous on the basis of a study of suicide among various cultural and ethnic groups in Constantinople.

Unfortunately, neither translation contains an index. *Suicide*, at least, reproduces the detailed table of contents of the French edition. *The Rules of Sociological Method* does not even offer that much.

HARRY ALPERT

U. S. Bureau of the Budget

*The Human Group*. By GEORGE C. HOMANS.  
New York: Harcourt, Brace and Company,  
1950. xxvi, 484 pp. \$6.00.

A foreword by Bernard De Voto and an introduction by Robert K. Merton impressively present *The Human Group* as a valuable contribution to sociological theory. This reviewer concurs, though he feels that Homans makes a far greater contribution to the teaching of theory than he does to the building of it.

Although *The Human Group* was not, apparently, designed as a text, its organization and style are admirably suited to classroom purposes. Homans develops the main ideas in a straightforward fashion. His use of terms, whether one would quarrel with his choice or not, is careful and free of ambiguity. His orientation is functionalistic, in the anthropological sense of the term, and Homans often refers to the work of Malinowski and Radcliffe-Brown. His analysis of specific groups in this regard is penetrating, with the result that for teaching purposes he manages to convey the point of view of functionalism in a much clearer and more down-to-earth manner than can be found in the writings of either of its two great exponents. Homans' treatment of social dynamics, moreover, is way ahead of theirs, and he has some constructive things to say concerning major points of difference between them.

*The Human Group* contains one other positive contribution of importance. Its author seeks to develop as many generalizations about the structure of interpersonal relations in groups as the

material he analyzes and his approach to it will permit. These he states in the form of propositions or hypotheses amenable to future test. Many of them are of necessity restatements of observations which already appear in the sociological literature. Others are new. A few examples will illustrate their range.

"A person of higher social rank than another originates interaction for the latter more often than the latter originates interaction for him."

"When two persons interact with one another, the more frequently one of the two originates interaction for the other, the stronger will be the latter's sentiment of respect (or hostility) toward him, and the more nearly will the frequency of interaction be kept to the amount characteristic of the external system."

"As the norms of a group decline in the degree to which they are clear to, and held in common by, all members of the group, so the ranking of members of the group will become less definite."

In the light of the many propositions formulated, Homans discusses such subjects as leadership, social control, avoidance taboos, the avunculate, social equilibrium, magic, social disintegration, and social conflict. In nearly all instances he has something valuable to say.

For example, he takes the proposition that "so far as A and B both originate interaction for C, the relationship between them is one of constraint, and interaction between them tends to be kept at a minimum," and suggests that the widespread phenomenon of mother-in-law avoidance by a husband is an expression of this principle. If this is so, then in societies where constraint and avoidance characterize the relationship between a man and his wife's mother, husbands should have authority over their wives and mothers over their daughters, whereas in societies where mother-in-law avoidance does not obtain, husbands should have little authority over their wives or mothers little authority over their daughters. This theorem can be tested cross-culturally. Indeed, one of the chief values of Homans' book is the statement of propositions from which theorems can be deduced for cross-cultural test.

In developing his concepts, Homans begins by defining what are for him the three elements of behavior: activity, interaction, and sentiment. In one way or another these three cover the what, how, with whom, why and wherefore of behavior in groups. The part of a group's behavior that is geared to its survival in its environment he calls the "external system,"

while his "internal system" includes "the elaboration of group behavior that simultaneously arises out of the external system and reacts upon it." In this he formalizes a common anthropological observation that culture, while of necessity related to the survival of society, is always elaborated beyond the obvious requirements for survival. The last of his initial concepts refers to that aspect of "sentiment" corresponding to what Ralph Linton has named "ideal patterns" for anthropology and which Homans calls "norms." In developing his propositions, he finds it necessary to introduce a number of other concepts, such as rank, liking, affection, constraint, with which he assumes the reader is familiar. In the sense of everyday usage there is no ambiguity here, and there is excellent precedent in social scientific literature for all of Homans' concepts, whether he defines them or not.

There is also excellent precedent for the manner in which Homans defines his concepts. Unfortunately it represents one of the major weaknesses of much social scientific theory. The method is to take a list of objects, words, or events, ask the reader to agree that they have some common element of meaning or form and then give the class of phenomena so designated a label. Definition by pointing and agreeing on terms is a very useful pedagogical device. It is a sure-fire way of eliminating misunderstanding. As it turns out, the concepts Homans derives in this manner are quite adequate to the task of illustrating the over-all interrelatedness of human behavior, attitude, and environment. But the classical conception of the universe as composed of the elements earth, air, fire, and water, arrived at in precisely the same way, was also adequate for certain purposes. Unfortunately, theory built on such conceptual foundations becomes in the end a juggling of labels, in which there is little concern for how they relate to scientific procedures.

The concept of social rank illustrates the problem. Social scientists are agreed that rank refers to a gradation of persons in a linear hierarchy. It follows, then, that the many specific behaviors by which ranks in a given society are differentiated must all manifest themselves as functions of one variable. If empirically they do not, our concept of rank is due for modification. As long as we lack methods for determining the interrelatedness of specific classes of behavior in this the mathematical sense of function, no reliable tests of propositions involving

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rank can be devised, nor can the concept have much concrete meaning for social science.

Such considerations in mind, this reviewer feels that *The Human Group* does not make a really significant advance in social theory, despite its genuine merits. This is not a reflection on its author, but on the present status of the field which he represents.

WARD H. GOODENOUGH

University of Pennsylvania

*The Foundations of Social Anthropology.* By S. F. NADEL. Glencoe, Ill.: The Free Press, 1951. xi, 426 pp. \$4.75.

This book, its author says, might better be entitled "Prolegomena to the Study of Society: being an Enquiry into the Nature of Sociological Knowledge." For it is neither a textbook nor a summary; rather, a statement of "the logical premises that underlie our knowledge of societies . . . and with the prerequisites, conceptual and technical, or any enquiry meant to lead to this knowledge." To the "Enquiry" Professor Nadel brings a rich and varied background. He is trained in the psychology of Köhler, Kafka, and Lewin; for many years he has worked as an anthropologist with Malinowski, Radcliffe-Brown, and their students, and had done much and excellent field work in Africa, (*vide A Black Byzantium*, and *The Nuba*); he is deeply indebted to sociology, particularly Durkheim, Weber and Parsons, and to the philosophy of science.

The Prolegomena falls into three sections: an introductory section on the place of anthropology in the social sciences, a section dealing with the materials of social life and the mode of their discovery, and a third on the meaning and nature of explanation in social science. I shall present the central argument as best I can, though this is no easy task. The inherent difficulties of the subject are not rendered easier by the author's circuitous style. Nor is the organization clear—Nadel does not indulge in topic sentences or other devices to assist the reader through his intricate argument.

In the first section Nadel argues the case for the science of society; the search for regularities and causes rather than historic antecedents.

The second section deals with the nature and materials of observation. After treating such "methodological" problems as the implications of the use of informant, the use of native language, and the "personal equation" in social research, he tackles the problem of the con-

scious and the understanding of motives in others (he accepts both). The "materials of observation" in Nadel's system starts with a unit, which is neither the individual nor the culture trait, but "man-acting," that is, the "standardized pattern of behaviour rendered unitary and relatively self-contained by its task-like nature and its direction upon a single aim" (p. 75). Such units are connected to other units by nexuses through either the *action* or the *individual* involved. These two classes of nexus lead to the two most important "dimensions" of the subject matter of social anthropology. The nexus of action leads to *institutions*, patterns of "man-acting" with common aim-content, and by extension to *culture*. The nexus through individuals leads to *groups* and the phenomenon of *society*. Nadel recognizes but gives rather short shrift to other dimensions, particularly language and "idea systems." These dimensions "meet" at the point of the "person," which might briefly be defined as individual-in-rôle. Institutions are "standardized modes of co-activity," characterized by purposiveness, perseverance, and a normative character (in a statistical sense). They may be classified into two ideal types, operative (such as food-getting) and regulative (such as legal). Groupings are collections of individuals standing in regular and relatively permanent relationships, set apart by "diacritical" institutions but having other institutional involvements. Nadel also discusses the nature of coherence and endurance, and the internal and external organization manifested by groups.

The section on "explanation" faces the following problems: the limitations of the model of the natural sciences; the use of quantitative methods; "common sense" understanding; the hierarchy of the sciences, and the origin of sociological entities. Nadel prefers causality to the mathematical "function." ("It is no good pretending that a full-blooded causal explanation is not more satisfactory than the emaciated version." p. 208). He distinguishes three interrelated causal systems: logical consistency, purposiveness of the agents of action, and reductionist causality.

The fundamental method of "experimental anthropology" is the establishment of co-variation by the "comparative method," a restatement of the method of Tylor, Radcliffe-Brown, and Murdock. It assumes (1) a preliminary hypothesis, (2) a meaningful nexus between the phenomena, and (3) the proper identification and differentiation of the social facts. The essential formula of covariance (if A, then B) must



take into account other variables either by noting them as common elements in the causal system, or alternate variables. The question of historic causation (i.e. diffusion) is not faced. He argues for the existence of "laws" of social behavior (as distinct from regularities within a single culture) though he does not endeavor to produce any.

Nadel's psychology lies in the middle ground between the instinct and the field-theory approaches, but leans rather to the latter. He recognizes finally only three categories of drives or "action potentials": pleasure-displeasure, equilibrium-tension, and conformity-shame. He utilizes the concept of "mental energy," or "capacity for work," which may be transformed into action, stored up, and diverted. He concludes the section with a discussion of function and pattern. Of the many meanings of functions he accepts the interdependence or requisiteness of one element to another. Pattern becomes fundamentally the subservience of myriad purposes to certain all-pervasive ones.

*Foundations of Social Anthropology* is important because it is the first effort by a social anthropologist to set forth either a scientific credo or a systematic approach to the field; because it presents a consistent scientific approach, unhampered by disciplinary cleavages; because it presents cogent supports for certain philosophical premises which do in fact implicitly underlie most analytic systems of modern anthropology; and because it redefines basic concepts and approaches in the field. The effort to bring into a single conceptual framework, concerns with cultural origins and modern "personality" research is in itself a valiant effort.

But, in a discipline and a school that has been built upon empiricism, it is disappointing to find the first integrated statement so completely divorced from factual content and reference. Ethnographic data enter only as illustrations, and rarely so. Discussion of the character of causality might well have given place to the nature of causes in specific socio-cultural situations.

Finally, while much that Nadel says is well worth the saying, the system itself is far from satisfactory. The inadequacy of the system can be pointed up by two fundamental omissions.

First, though there is a recognition of aims as manifested by persons in the society, and of needs in man, the nexus between these, and their relationship to production of goods by utilization of the environment is not developed. He treats primitive social systems with an inadequate

awareness of the implications of the imperatives of food getting (or, for that matter, sexual satisfaction) in formulating social behavior. Though he accepts the idea of cultural evolution, he does not see it as a dimension in the formation of social systems.

Second, his social system does not adequately appreciate the implications of conflict in society. Aims, which loom large in his theoretical position, are viewed as cultural aims held in common by members of a society. The existence of conflicting aims, and the conflict over the achievement of common aims, both of which are of greater importance to primitive social system than anthropologists have appreciated, and which have such far-reaching consequences for the nature of institutions, are not given a place in his analysis. So, too, with organizational imperatives in general. Nor are such phenomena as "the definition of the situation" or "unanticipated consequences of social action" either brought into the system or explained away.

In summary, Nadel has brought great erudition to the task of understanding human society, and if he has failed to offer a cogent and satisfactory theory, he has nevertheless shed light on many knotty problems and cleared away some of the tangle of false conceptualization. For this, he will be read by every student of social theory.

WALTER R. GOLDSCHMIDT

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*Social Psychology at the Crossroads: The University of Oklahoma Lectures in Social Psychology.* Edited by JOHN ROHRER and MUZAHER SHERIF. New York: Harper & Brothers, 1951. viii, 437 pp. \$4.00.

The conference on the current condition of social psychology was held in April 1950 at the University of Oklahoma, but the crossroads reference is to the issue of whether or not psychologists who take responsibility for social psychology are to mean business by the "social" aspect of it. The present contributors have such intentions, but they acknowledge that not all social psychologists do. Texts in the field have in fact included some formulations that differ little from those of purely individual psychology, and some that make gestures toward being social but which seem unable to conceive of the social element as being more than an artificial veneer over the basic realities of physiological processes. As Newcomb states (p. 32) "... most social psychologists of pri-

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marily psychological persuasion take no systematic account of the facts of the social environment in which human organisms live . . . they minimize or even ignore the nature of the social structure of which their subjects are members. They often speak and write as if the differences between the human and non-human environments could be ignored. The result of this comes perilously close to being a human social psychology without people."

The Oklahoma conference, however, was composed of those who do take the social part seriously, even to the extent of following some of the sociological literature and of inviting sociologists to take part in the conference. These psychologists are not necessarily recent converts, but they are recently growing in prominence and influence, and it is to their credit that they are also renouncing the jurisdictional sensitivity and professional myopia that have been such a drag on much of social psychology.

The symposium opens with a modestly expressed statement on the contribution of genetics, in which it is stated (p. 53): "Geneticists cannot pretend to have made conspicuous contributions toward the analysis of human behavior patterns, and it is not likely that they will do so in the very near future." Nevertheless, they consider that they have some findings which have "real implications for the social sciences." These implications are rather negative, as far as the role of heredity in behavior is concerned—certain gross defects which limit behavior appear to be inherited, but apart from that these geneticists think that normal persons are on the whole not nearly reaching the potential limits of their achievements. If they ever should, then the levels of achievement would at least in part reflect hereditary differences.

An almost revolutionary paper by H. F. Harlow, based to a considerable extent on animal research, calls into question a number of notions about learning. The drive theories of motivation do not stand up very well, even with monkeys, and Harlow makes a good deal of social motivation which does not spring from any physiological need or drive at all. Here is an important break with the conception of physiological domination of motivation—a point of view not new to all of us, but long resisted by most specialists in animal research, who have apparently felt it important to justify their research operations partly by the claim that their animals reveal the same principles that govern human behavior.

The bulk of the volume contains theoretical discussions of matters concerning social interaction, and includes among other topics some stimulating reflections on the following matters: the relation of person to group; social attitudes; norms; the social control of perception and memory; enculturation; the concept of self; roles; status; interaction in small primary groups; leadership; and multiple group membership. There is frequent reference to current research, with bibliographical lists at the ends of some of the papers, all of which have an up-to-date flavor.

Two studies merit special comment. Roger C. Barker and Herbert F. Wright present "The Psychological Habitat of Raymond Birch" which is in effect a classification and interpretation of every one of the 712 things that a seven-year-old boy did from the time of awakening to the end of his day, on April 26, 1949. The Lewin influence shows in this study, with judgments on valence, reality-phantasy level, centrality of need, and the like. When Raymond speaks to his dog, for example, it is judged to be "reality completely" apparently without consideration of the question whether the dog can really understand English, or whether Raymond sometimes greets animals automatically. In other respects as well not all would agree that the categories are as objective as they appear, but it will be interesting to see what uses can be made of such an approach.

Sherif presents a preliminary account of an experiment on boys at a summer camp. By separating boys who had built up incipient friendships into two artificial rival groups, he made them build up strong loyalty to the new groups and bitter conflict with former friends now in the rival category. This may not surprise everyone, but it should for one thing present the advocates of the catharsis concept a problem of defense of their views. It also provides an account of a powerful and dominant kind of motivation that began in a purely artificial decision and penetrated into the boys from social sources alone. The full report, which is to appear in a forthcoming book, will be pondered from a number of points of view.

In view of the evidence of respect for and desire to cooperate with sociologists the latter will refrain from protest concerning the reference on page 394 to "leading sociologists like Durkheim" in the present tense. We now honor Durkheim as psychologists honor William James but he does not speak for our science today. There is another trivial but amusing

anachronistic item consisting of a statement by a C.C.N.Y. professor, "When I, a City College person, talk with a representative of another college, to achieve high status, I'll talk basketball." It may now be time to recommend that basketball fans turn to social psychology.

ROBERT E. L. FARIS

*University of Washington*

*The Psychology of Intelligence.* By JEAN PIAGET. (Translated from the French by M. PIERCY and D. E. BERLYNE.) London: Routledge and Kegan Paul, 1950. 182 pp.

The International Library of Psychology, Philosophy, and Scientific Method after an interval of many years has published another Piaget volume. Since his first five books appeared in English, Piaget has written approximately ten books. Most of them are reports of empirical research on children. All are guided by a systematic body of theory. The appearance in translation of *The Psychology of Intelligence* (originally *La Psychologie de l'Intelligence*, 1947) may stimulate American social psychologists to read the untranslated writings. It may also serve to correct the notion that because conflicting conclusions have been reached about several of his earlier, and generally minor, hypotheses the bulk of his systematic research can be disregarded.

In this small book, Piaget summarizes his general position. There are levels of adaptive behavior. There is a functional, but not structural, continuity between higher and lower levels. So called "intelligent" behavior is an extension of all adaptive processes, and is not to be conceived as something apart from lower levels of behavior.

Piaget argues for studies of the genetic development of intelligent behavior in the child. In doing so he suggests that behavioral organization at various levels be thought of as temporary equilibria of "operations." His discussion of the higher organizations centers around logical operations and propositions. Perceiving as learned activity comes in for extended treatment and perception as innate is criticized.

The most valuable sections for the social psychologist are those that deal with "Habit and Sensorimotor Intelligence" and "The Growth of Thought." Piaget conceives of the child as moving through stages of behavioral organization (he calls them stages of thought), each marked with determinable characteristics. After the initial sensorimotor activity, there is a period (from about 1½ to 4 years of age) of

preconceptual thinking. Next comes (4 years to 7-8) a period when the child's thinking is marked by the absence of logical operations. This type of thought is termed "intuitive." During the years 7-8 to 11-12, the child learns to think logically, on a concrete level, about objects that can be manipulated or known through the senses. Finally formal logical thinking—operating on operations—is perfected.

Treatment of the child's passage from sensorimotor to thinking logically on the concrete level is perhaps the most original and suggestive part of the volume. Sensorimotor behavior is discussed in six sub-stages, during which we observe the gradual differentiation and coordination of responses. In the last sub-stage we witness the appearance of representations or images which refer to absent objects and events, "independently of the advent of language." Piaget points out that Kohler's apes evidently reached this level. Piaget calls these representations, "enacted images" or "symbols" to distinguish them from "signs" which are collective representations (i.e. Mead's significant symbols). The acquisition of language coincides with but does not explain the appearance of these individual symbols. The initial learning of words is discussed as preconceptual thought, and the implication is that while behavior is now partially verbally regulated, the child's words cannot be thought of as genuine concepts. Yet preconceptual thinking marks an advance in the child's development since objects are in some sense classified and thus acted toward from a more general and socialized viewpoint. Preconceptual thought in its later stages merges into intuitive thought, which later marks a gradual coordination of representative relations and a growing adequacy of conceptualization. During the intuitive period a rudimentary logic is observed at play (whose mechanisms are discussed briefly here, but extensively in previous books on the child's notions of space, time, geometry, physical quantity, etc.). True logical operations such as logical addition and multiplication are, Piaget maintains, lacking.

Whether or not Piaget is correct in his interpretations of preconceptual and intuitive thinking, investigations of the child's socialization are going to have to take his hypotheses into account. Social psychologists who are interested in the development of roles, identifications, motives, and self will find this book a rich source of hypotheses and suggestions.

ANSELM STRAUSS

*Indiana University*

*A History of Medicine, Volume I: Primitive and Archaic Medicine.* By HENRY E. SIGERIST. New York: Oxford University Press, 1951. xxi, 564 pp. \$8.50.

This volume, publication number 27 of the Historical Library of the Yale Medical Library, is the first of a projected eight-volume history of medicine which represents the culmination of more than twenty-five years of preparation by the foremost living medical historian. As Dr. Sigerist says in his Foreword, "Every book and every paper I wrote was meant to be a preparatory study, and many of my lecture and seminar courses were given not only for the benefit of students but to fill some gap in my knowledge or to review a period or a set of problems about which I had doubts." He also states that he studied fourteen languages in order to consult the original texts. In 1947, Dr. Sigerist retired from a distinguished teaching career, and Yale University is to be congratulated on its foresight in making him a Research Associate with professorial rank on indefinite leave of absence to enable him to devote full time to this monumental piece of "medical historiography." The breadth of scholarship which is apparent throughout the book is documented by an extensive and usefully annotated bibliography.

The first volume sets a high standard of lucidity, informativeness, and readability. So thoroughly has Dr. Sigerist mastered his material that he has no need of technical jargon, and his writing will appeal to the layman as well as the medical specialist. Apt analogies, vivid illustrations, and an interesting style makes for quotable passages on nearly every page. In addition there is a large section of photographs and drawings.

The first quarter of the book is the introduction to the entire eight volume history, including a summary of the development of medical history, which, according to Dr. Sigerist, "is first of all history, a historical discipline like the history of philosophy, the history of art, or the history of music." It is also one of the youngest historical disciplines. Its field of study is "health and disease through the ages, the conditions for health and disease, and the history of all human activities that tended to promote health, to prevent illness, and to restore the sick." This involves, for any culture, a consideration of geographical, economic, and social conditions, religious, educational, and philosophical ideas, as well as therapy and health. Dr. Sigerist has long been concerned with what he terms "social medicine"; he calls medicine a social science

since its aim is to keep individuals adjusted to their environment, and considers it one link in a chain of social welfare institutions.

The remainder of the volume is divided among primitive medicine and that of ancient Egypt and Mesopotamia. Dr. Sigerist sees no necessity for discussing primitive medicine first; according to him its "timelessness" could have put it in volume VIII instead of volume I. This statement would seem largely to ignore the preliterate background from which the archaic civilizations developed, and in practice Dr. Sigerist often points out parallels and survivals of primitive medicine in civilized times. The discussion of primitive medicine is, as far as this reviewer knows, the fullest and most balanced account to be found in any general medical history. The archaeological evidence is carefully reviewed, and a wide range of ethnographic data is assembled. In view of Dr. Sigerist's familiarity with Sumner and Keller's *The Science of Society*, it is perhaps surprising that he does not make more use of their concept of automatic selection instead of what seems like overreliance on human instincts to account for the beginnings of medicine. Primitive medical treatment is discussed as the logical consequence of primitive theories of disease, and Clements' important work on concepts of disease is utilized. The unconscious psychotherapeutic value of much primitive treatment is adequately covered, although the actual level of medical and surgical skill achieved by a number of preliterate groups is perhaps minimized. The sincerity and social usefulness of the medicine man is very well developed, and a sound cultural anthropological approach characterizes the whole discussion.

The sections on ancient Egypt and Mesopotamia follow the same general outline. The historical setting is first described and then those factors particularly relevant to health conditions. The archaeological material is admirably summarized, with generous credit given to a number of experts. The place of medicine in the total culture, the patient-physician relationship, and the technical medical knowledge are clearly presented, and illustrated with extremely well chosen translations from papyri and cuneiform tablets. In comparing the two countries "we find significant differences in religious concepts, in the administration of justice, in artistic styles, and in general customs. In medicine, while the elements were the same, yet their emphasis was different. Egypt, so far as our present knowledge goes, developed the empirical and rational side



more highly and earlier than Mesopotamia, where magic and religious practices maintained their dominating influence to the very end."

There are points of emphasis throughout the volume. Treatment is shown to be logically related to theory of disease in all the cultures discussed. The psychosomatic nature of medicine is emphasized, with the effects of guilt and anxiety in causing discomfort and the therapeutic values of confession, atonement, and countermagic. Where the evidence is available, an evaluation of the treatment from the pharmacological or physiological standpoint is made. Respect is shown for the religious ideas of other peoples; Dr. Sigerist has no patience with those ethnocentric writers who dismiss religious or magical practices as "absurd superstitions." The availability of medical services to all levels of the population is a point of interest, as is Dr. Sigerist's discussion of society's need to protect itself from possible misuse of power by medical experts.

The volume has overtones reminiscent of Toynbee and Spengler. Cultures are seen as entities, arising, flowering, and decaying, passing on something of their accumulated experience and technical knowledge to become the starting point of new developments. What is uniquely Dr. Sigerist's own is his placing of medicine in its cultural setting, relating it not only to religion, as many previous medical historians have done, but to art, literature, government, trade, and economics. Subsequent volumes are planned for Oriental, Biblical, Greek, medieval, and modern medicine. If they succeed equally well in presenting what may be called a cultural view of medicine, they will greatly enrich our understanding of man's persistent efforts to understand and overcome pain and disease.

This volume is warmly recommended to social scientists, as well as to the medical profession, and the general reading public. It is unfortunate that its somewhat formidable price will tend to limit its circulation.

ELIZABETH A. FERGUSON

Skidmore College

*The Burden of Diseases in the United States.*

By ALFRED E. COHEN and CLAIRE LINGG.  
New York: Oxford University Press, 1950.  
vii, 129 pp. 5 color charts in separate slip case.  
\$10.00.

This is a study of and a commentary on the dramatic changes that have taken place in the incidence of disease in the United States in the

past hundred years. The senior author is a very eminent physician who is internationally known for his research work in cardiovascular disease. He was a long-time member of the Rockefeller Institute for Medical Research. His co-author, Miss Lingg, is a staff member of the American Heart Association and collaborated with him while she was secretary to the latter Association's former Committee on Research.

Most of the discussion of the natural history of disease in America is based upon an analysis of the raw data supplied by the Bureau of the Census covering the years up to and including 1940. There is a useful, though elementary, treatment of population change and death rates. Among the cogent comments made is the following: "On all sides have been heard statements to the effect that the death rate in the past fifty years has been decreasing. To make statements like this is to prepare the way for disillusion. Surely 100 per cent of people die; at certain periods, coincident with certain events or the introduction of certain procedures and arrangements, the rate may fall. But for the fall there must at some time be compensation, until a new equilibrium is established. If deaths are obviated in infancy or early adult life, they must take place in later decades. The duration of life, its expectancy, as we learn from actuaries, has in fact increased. Does not the situation amount to this? We have been saved from death in youth, to live, it may be, laborious days, and to die in the end by more lingering mechanisms."

There is an interesting chapter on the leading causes of death in general and also with reference to the various stages of life. This is followed by brief discussions of sex and race differences in the forces of mortality. A large section is devoted to an analytical discussion of the changes in the mortality curves from specific disease conditions.

The authors state: "A study that purports to give an account of the state of health—or, better, the amount of illness in the nation—is inescapably incomplete if it devotes itself only to the facts concerning mortality. Without doubt the suffering that mortal illnesses cause is only a small fraction of the total burden the people support." For this excellent reason, a section is devoted to a description of the salient findings of five of the best known morbidity surveys in the United States. The need for long-term observations and studies of identifiable populations from birth to death is stressed.

This book is lavishly illustrated by graphs of divers kinds. Most of these are extremely effective.

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tive. The colored charts, however, though very striking, are rather jumbled. A good bibliography is appended.

The level of approach to the subjects discussed is sometimes curiously uneven. The concept of the death rate as distinguished from the number of deaths is described in elementary fashion. On the other hand, with regard to other technical concepts such as the relative importance in the aging process of changes in blood vessels as opposed to changes in the tissue cells fed by these blood vessels, a rather advanced understanding of terms and biological philosophy is taken for granted.

To those who are new to the study of the health status of the people of the United States, this book provides a fairly well-rounded discussion. To others, however, it offers little that has not been previously available in the literature.

CECIL G. SHEPS

University of North Carolina

*A Classified Bibliography of Gerontology and Geriatrics.* By NATHAN W. SHOCK. Stanford: Stanford University Press, 1951. xxvii, 599 pp. \$15.00.

*Trends in Gerontology* By NATHAN W. SHOCK. Stanford: Stanford University Press, 1951. ix, 153 pp. \$2.50.

Good organization and clarity of style characterize *Trends in Gerontology*, written by the Chief of the Section on Gerontology of the National Heart Institute, under the United States Public Health Service. With its much larger companion volume, *A Classified Bibliography of Gerontology and Geriatrics*, it brings the student of gerontology abreast of trends and research through 1948. The emphasis of *Trends* is, first, upon research into the fundamental process of aging and into the capabilities of old people; second, upon programs of action to give productivity and independence to people over 65 with whatever supportive care is needed as the years accumulate. The first eight chapters summarize briefly trends in population (increased life expectancy); employment (decreased percentage of older persons in the labor force since 1890); income (increased dependency upon pensions and old age assistance); health maintenance (beginning awareness of need for definite programs); living arrangements (special non-institutional housing projects for the minority who do not maintain their own households, and better supervision of institutions); education (first steps in education both

for oldsters and for social workers and researchers who deal with various problems of the old); and community programs (chiefly recreational). Original inquiries as well as published reports form the basis for these chapters. Not only are the main trends listed but pertinent questions are asked regarding long-range effects, and suggestions are made for originating or improving practical programs. For instance, when Dr. Shock points out that reduction in chronic disease will lighten the economic burden of the care of the aged, he also stresses that longer life, even without chronic disease, poses a long-term economic burden unless research discovers what productive tasks old people can perform, and practical programs make such work on a paid basis available.

Chapters 9 and 10 list with considerable specificity types of research and the staff members engaged in each type, and the centers of research with long-term organized programs of research in gerontology. The thoughtful sociologist will be struck by the fact that whereas eight pages are required to list the research under way in structural changes, biochemistry, and physiology, one and one-half pages suffice for research in psychological and socio-economic problems. Thirteen institutions have long-term organized programs for research in physical problems, but only two (University of Chicago and Moosehaven, Florida) have research programs in sociological or psychological processes.

The last two chapters emphasize the great need for an extended and accelerated program of research, bringing all aspects of research into an integrated program, which would also include teaching and service to the aged. The time is ripe, Dr. Shock says, to induct into the program young researchers committed to long-term studies in gerontology to replace the present tendency of old-age research to be a sideline to some other interest.

The *Bibliography* is a massive volume of 599 pages and over 18,000 references originally published in fourteen languages. The references are classified in sub-groups under the general headings of Gerontology, General Orientation; Biology of Aging; Organ Systems; Geriatrics; Psychological Process; Social and Economic Aspects; and Miscellaneous. The volume demonstrates the extensive research already accomplished, largely by isolated workers, in this "new" field of gerontology and geriatrics. It will be the first volume to which the professional research worker will turn in planning and carrying out new research.

Recognizing that in any growing field a bibliography is already out of date by the time of publication, Dr. Shock arranged with the *Journal of Gerontology* to continue the bibliography past 1948 by publishing references to research under the same classification as that used in his *Bibliography*.

All those interested in research on the processes and problems of aging are indebted to Dr. Shock for two volumes that will be useful not only in a practical way but also to promote much needed integration of the various disciplines now centering attention upon research in gerontology.

RUTH SHONLE CAVAN

Rockford College

*Of Societies and Men.* By CARYL P. HASKINS. New York: W. W. Norton & Co., 1951. xiv, 282 pp. \$4.50.

The versatile author of this challenging book is a distinguished student of the social insects, as well as of biophysics and many other things. Similarities between insect and human societies have been stressed *ad nauseam* by entomologists no less than by uncritical writers whose fallacious reasoning is more excusable. Haskins is no less aware of the differences than of the similarities, and his book deserves a wide audience among social scientists.

Its principal thesis is as follows: Three sorts of social groups are found among sub-human animals—the family, the integrated society (of the social insects in particular), and the associative society (of herd, flock, etc., characteristically vertebrate types). The integrated society is an almost inevitable outcome of the family society, given the twin evolutionary trends toward complexity and specialization that are shown to exist throughout the organic world and probably in non-living matter as well. The associative society, by contrast, is not an automatic result of family life, and has properties quite different from those of the integrated society, including a tendency opposed to integration and specialization. Now human culture has a unique kind of society, representing a balanced mixture of properties derived both from integrated and associative societies. Man's salvation therefore resides in recognizing the fact, and particularly in maintaining the open system of the associative type, for to emphasize the integrative side to the exclusion of the associative is to hasten totalitarianism. The danger inherent in the entomological fallacy is that if man thinks he is like

an ant, he is perfectly capable of directing his social evolution in such a way as to become an ant.

So heartening and persuasive a plea for democracy as a biological imperative will not go unchallenged, and there will be many (beginning with a disingenuous reviewer in *The New York Times*) who will pounce on the weakness of argument by analogy, which admittedly pervades the book. That cannot be helped; the essence of the comparative viewpoint, which Darwin's work shows not to have been wholly unrewarding in biology, is that some kinds of analogy amount to homology. It is hard enough to distinguish one from the other in biology, and even harder in sociology; but the social scientist will make a fatal mistake if he gives up the attempt because a philosopher tells him that Aristotle thought it was wrong.

Haskins' book is rich in interest for a biologist as well as for a sociologist. His examples from the insect world are fascinating and unhackneyed, and the range of his reading is enormous. A minor complaint is that there are no illustrations. The price set by the publishers, absurdly high even by present standards, might be justified in another edition if some drawings and photographs were added. This would enhance its value as a textbook as well as its popular appeal, for Haskins' literary style, though direct and free from orotund vapidity, would not be the worse for a little embellishment.

E. S. DEEVEY

Yale University

*The Human Species: A Biology of Man.* By ANTHONY BARNETT. New York: W. W. Norton & Co. Inc., 1950. x, 280 pp. \$3.75.

This book is an attempt to relate some of the facts of biology to the human and political problems which face us at the present time. The volume is divided into three major parts concerned with heredity, physical diversity, and public health. Part One deals with genetics, the interaction of heredity and environment, the importance of twin studies, and includes a brief account of Mendelian theory. There follows a remarkably condensed but clear account of fertilization, implantation, and development. In Part Two are considered the causes of evolution, apes and fossil men, the races of man, and lack of significance of race in hybridization, and studies of crime and intelligence. Eugenics is dismissed, and the waste of genetic ability caused by lack of environmental opportunity is de-

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plored. Part Three is introduced by an account of the evolution of society from savagery to socialism. Then follow chapters on the quantity and quality of food, public health, and the growth and decline of populations. There is a bibliography of five or six books for each chapter, with a note on each book. The titles are excellent secondary sources, cover an extraordinary range of territory, and the author is to be congratulated on his success in abstracting and in bringing these diverse materials to focus on a few important human problems.

Barnett's interest in human biology stems from his concern with social and political problems. He begins his final chapter with a quotation from Karl Marx. "The philosophers have only interpreted the world in various ways; the point however is to change it." According to Barnett, the task of human biology is to assist in the change from capitalism to socialism, to minister to the immediate needs of man, and to waste no time looking for truth. Specifically, in this volume Barnett defends Lysenko, disparages pure science, and calls for the practical application of science following the pattern of the U.S.S.R. He is unable to use genetics to clarify the race problem because he is too concerned with politics to see the relation of chapter three to chapter six. The human genetics presented is, of course, a derivative of the fundamental work on flies, and there would be no science of genetics at all if people of Barnett's beliefs had been running the world.

All will agree that understanding of human genetics, nutrition, and disease has progressed far beyond present application, and that this knowledge should be used to improve the lot of mankind. But it is debatable whether Marxist materialism will advance either biology or its application to human problems. The wise use of this information will take a more sophisticated social science than a series of evolutionary stages leading from savagery to socialism.

S. L. WASHBURN

University of Chicago

*The New You and Heredity.* By AMRAM SCHEINFELD. Philadelphia: J. B. Lippincott Co., 1950. xxii, 616 pp. \$5.00.

This is an outstanding example of the successful popularization of a difficult subject. Starting with descriptions of the sperm and egg, the reader is quickly introduced to present ideas on fertilization, chromosomes, cell division, and the determination of sex. The discoveries of Mendel

and Morgan are described and the idea of the gene introduced. Then the bulk of the book deals with the interrelation of heredity and environment in the production of: form and color of eye and hair, skin color, many physical features, gross size and shape, twins, defective heredity, functional and mental failure, blood types, longevity, musical talent, genius, behavior, criminality, sex life, evolution, race, population, and eugenics. There is a list of selected readings on the major topics covered and the author lists over 50 of the nation's leading scientists who read the sections of the manuscript concerned with their specialty. The illustrations are good, the writing excellent, with a minimum of technical vocabulary.

Three major themes run through the book: First, the way genetics helps us to understand human structure and behavior in health and disease; second, the way genetic understanding eliminates misapprehensions (a truly vast number of popular misconceptions are neatly dismissed); third, the way heredity and environment are both important, the practical problem being their relative roles in a particular situation.

A general criticism which the reader of *The New You and Heredity* might bear in mind is that the book leaves the impression that more is known about human heredity than is the case. This criticism applies particularly to the tables that forecast what a child may look like, even though these are worded in a general way and the possibility of exceptions noted. Surely it is misleading to speak of "nose genes" when not one single gene has been identified which affects the form of the nose. The tables of eye-color, hair color, and hair form (especially statements like "the gene for curly dominates wavy," etc.) are likely to convey the idea that the genetics of these features is well understood, which is not the case. Scheinfeld points out that these predictions are based on averages and that one child "might be the exception." But the very point of Mendel's great discovery and the science which developed from it is that genes do not average. The difference can be made clear by comparing the statements of the inheritance of the blood types (Chap. 28) to those on eye color. If both parents are homozygous A, the children are A, the only exception being if a mutation has taken place. It is frequently impossible to predict eye color of the child from the eye color of the parents. The blood groups are the only large group of normal human traits for which: (1) the actual mode of inheritance is known; (2) investigations of hundreds of thousands of



cases have been made; (3) there is practical importance; (4) the frequency in various races is known; (5) knowledge is increasing rapidly. There would be much to be gained by starting a book on human heredity with an examination of those traits whose mode of inheritance is understood in detail.

Although the author is careful to stress that environment and heredity are both important, I feel that the role of the environment has been underestimated in the discussions of intelligence, behavior, and the influence of the mother on the fetus. The tremendous effects of diseases or dietary deficiency of the mother on the unborn child (hydrocephalus, mongoloidism, cleft palate, eye defects, many "congenital" anomalies) completely alter ideas concerning the relation of the mother and child. Much of what was regarded as defective heredity may now be explained by the maternal environment.

Two books mentioned in the bibliography evidently arrived too late for incorporation in the text. The material in *Unraveling Juvenile Delinquency* by Glueck and Glueck (1950) would have helped to give very concrete evidence to demonstrate the overwhelming role of environment in the production of crime. A table from Boyd (*Genetics and the Races of Man*, 1950) would show how races can be described in terms of frequencies of known genes, thus making clearer both the concept of race and the relation of genetics to racial classification.

S. L. WASHBURN

University of Chicago

*Patterns of Sexual Behavior.* By CLELLAN S. FORD and FRANK A. BEACH, with an Introduction by ROBERT L. DICKINSON. New York: Harper & Brothers and Paul B. Hoeber Inc., 1951. viii, 307 pp. \$4.50.

Simply and clearly written, with no axe to grind but to present the facts, *Patterns of Sexual Behavior* is a book which, for the first time, places the study of sex in a broad scientific perspective. In this work Professors Ford and Beach—the former an anthropologist, the latter a psychologist—have amassed and analyzed an enormous body of data on sex behavior (by which they mean exclusively "behavior involving the stimulation and excitation of the sexual organs") within a threefold frame of reference. The first is cross-cultural; the second, evolutionary; the third, physiological. The results of their efforts, which have important theoretical, methodological, and prac-

tical implications, clearly demonstrate the fruitfulness of a cross-disciplinary approach to the problems of human behavior. If widely read, as it should be, this book will undoubtedly stimulate more thorough research on a much neglected phase of human relations; and it will also create a healthy impact on attitudes toward sex—and on sexual behavior itself—within our own society. It may be recommended, therefore, to specialist and layman alike.

The authors' purpose was not to produce an encyclopaedia on sex. As they themselves state, their first aim was "to describe the basic sexual patterns of human beings and their closest [mammalian] relatives." Within the limitations of their data, which are avowedly many, this aim has been fulfilled with care and precision. Materials on sex behavior from 190 societies—a geographically representative sample (including American society) selected from the Human Relations Area Files at Yale—have been compared with each other and with those of a significant number of animal species closely related to man.

The data of the book, introduced by a section on "The Task and the Methods," are presented in a series of chapters, each with a careful summary, dealing with the following material: the nature of coitus, types of sexual stimulation, circumstances for coitus, attracting a sexual partner, sexual partnerships, homosexual behavior, sex relations between different species, self stimulation, sexual development in the individual, feminine fertility cycles, and other physiological factors in sexual behavior. A concluding chapter entitled "Human Sexual Behavior in Perspective" admirably summarizes the findings and some of the implications of the study.

Attention can here be called to but a few of the important contributions of this book. In the first place, by having examined their data in a cross-cultural perspective, the authors have not only demonstrated the wide range of sexual behavior that exists among human groups throughout the world but they have, at the same and for the first time, statistically documented for their sample the presence or absence of specific sexual practices, as well as attitudes toward them. With respect to this last point, for instance, American society is found to be one of the minority that disapproves of all forms of homosexual behavior. "In 49 (64 per cent) of the 76 societies other than our own, homosexual activities of one sort or another are considered



normal and socially acceptable for certain members of the community" (p. 130).

In the second place, by having made a cross-species analysis of their subject matter, the authors have called attention to a number of patterns of sexual behavior which they conclude are strongly affected by evolutionary factors. Self stimulation of sexual organs and homosexuality, for instance, are found to be practiced universally by man and his closest relatives; therefore an inherent biological tendency toward such activities is probably found in all mammals including human beings. It is at this point that considerable argument could be raised. While it is perfectly valid to compare man and the animals on a biological level, it is questionable whether this type of analysis is of much significance for an understanding of sexual behavior on the human level where motivation toward specific activity is largely, if not exclusively, cultural. In other words, it is highly doubtful whether we can base valid generalizations about specific patterns of human sexual behavior on specific patterns of infrahuman sexual behavior where motivation toward specific activity is largely, if not exclusively, biological. Unfortunately, the authors do not deal extensively enough with the symbolic aspects of sexual behavior, which are the most significant as far as man is concerned and which do not exist below the human level. One's reading of this book, for example, or one's going to a movie, might deeply affect his sexual behavior, but the same could not be said of an ape, an elephant, or a rat.

A third contribution of this book is that it brings together a good deal of modern physiological data on sex about which most social scientists have but limited knowledge—data on physical maturation, on the sexual development of the individual, on sex cycles of men and women, on ovarian and testicular hormones, on drugs and sexual behavior, on sex differences between men and women, on the functions of the nervous system in sexual behavior, etc.

Finally, *Patterns of Sexual Behavior* is a plain invitation to social scientists to embark on a field of research which, with few exceptions, has been badly neglected up to now because of our own cultural biases. The paucity of cross-cultural data on sex, for example, as revealed by evidence in this book, clearly indicates that most anthropologists have been preoccupied (in their reporting, at least) with other matters. So sparse and subjective are many of their data—and collected with little adherence to rigorous

method—that one could seriously question whether they can be validly employed at all for comparative purposes. Nevertheless, by calling attention to such weaknesses in our data and by bringing together what reliable cross-cultural, evolutionary, and physiological information we have on sex, the authors have pointed the way toward a more scientific approach to the theoretical and practical problems of human sexual behavior.

ALLAN R. HOLMBERG

Cornell University

*Race Relations: The Interaction of Ethnic and Racial Groups.* By BREWTON BERRY. Boston: Houghton Mifflin Company, 1951. xiii, 487 pp. \$4.75.

Professor Berry has written a text that sets high standards of excellence in comprehensiveness, readability, and scientific accuracy. His general plan is to begin with groups or peoples of different cultural or racial origin and then to chart the course of their contact and interactions in terms of "standard" sociological concepts such as conflict, assimilation, segregation, stratification, etc. The author treats each topic with copious examples drawn from Africa, India, Latin America, Hawaii, Indonesia, Australia, and the continent of Europe as well as from the United States. The book is truly international in scope and thus avoids the insularity of a purely "American" approach.

Charts, diagrams, and illustrations abound; the many photographs employed give richness and vividness to the text. The introductory section includes a keen and judicious account of the meaning of race, race differences, and race prejudice, again in international terms. Yet Berry is cautious in evaluation; he comments, "Unfortunately, our data on race relations in other lands are sketchy. We are in great need of comparative studies of intergroup relations in the various biracial and multiracial areas of the world. Only when such materials are available will it be possible to develop a science of race relations" (p. 20). In like vein he rejects the race-relations cycles of Park, Bogardus, and Brown as too particularistic (pp. 134-138).

It is unfortunate that the author continues to use the traditional term "race relations" both in the title and throughout the book as a whole. Even though he explains precisely what he is doing (pp. 71-76), the title itself may help to keep popular concepts alive. This nemesis overtakes the writer himself when he refers to those

expelled by the Nazis by saying that "not a few of them belonged to the approved race" (p. 36). And if we believe, with Berry, that it is "unpardonable" to refer to the Jews as a race, it may grate on our sensibilities to open the chapter explaining the anthropological concept of race and find on the opposite page a picture of orthodox Jews performing their ritual in the synagogue. With minor modifications the term "minorities" could avoid these complications, as the author seems to realize in later chapters where he employs it more frequently.

At the outset, Berry announces that he will present an objective account without taking a stand on the values involved. Though he recognizes that as a person he may find this difficult, he states his intentions clearly. However, this prevents him from examining his value presuppositions any further, and from succeeding in his quest for objectivity, quite as Myrdal predicted would happen in such a case. The observant reader will notice signs of moral indignation in the stories of mass expulsion of Cherokees and Japanese Americans (pp. 206-214) by noting such affective terms as "revolting," or "injustice . . . colossal" scattered through these pages. No such terms appear in connection with the slave trade in Negroes. Are any inferences possible when the author departs from his stated objective in one case and not in the other?

A few minor errors occur here and there, such as "Porto Rican" instead of "Puerto Rican" (p. 141) and "Greek Orientals" instead of "Greek Orthodox" (p. 224). Berry asserts that the Czechs and Slovaks prefer the term Czechoslovakia with a hyphen to indicate their separation (p. 391), while it is the Slovaks alone who make this demand. He reports that marriage across religious lines is increasing, whereas other evidence—e.g. Marcson, in *Social Forces*, October 1950—shows that it is reaching a standstill.

On the whole there are few important omissions. But the author slights ideological elements like the history of racism; he also neglects civil rights programs, the continual extension of F.E.P.C., Community Relations Boards, and new Supreme Court decisions that in the U. S. are changing the pattern of inter-group relations. Nor does he mention inter-group education. Actually, another "standard category" which Berry omitted for reasons unknown is that of *cooperation*. This could be treated quite as objectively as the others.

The fact remains that this is a text which will give keen satisfaction to both teacher and student. For those who prefer the topical or categorical approach, *Race Relations* is by far the best introduction in the field. It bears the mark of sound and exacting scholarship, thorough acquaintance with the literature, and sagacious choice of provocative illustrations that will stimulate reflection. The extensive bibliography is first rate. Finally, the publishers have given the book an attractive format that enhances its value.

R. A. SCHERMERHORN

Western Reserve University

*Statement on Race: An Extended Discussion in Plain Language of the UNESCO Statement by Experts on Race Problems.* By ASHLEY MONTAGU. New York: Henry Schuman, 1951. xi, 172 pp. \$2.00.

In this volume Professor Montagu outlines the steps leading to the formation of a UNESCO committee which prepared a statement on race to be made the basis of an international educational campaign based on empirical findings agreed upon by biologists and social scientists of many nations. Although Soviet scholars are not in the list (and this fact is neither explained nor discussed), all members of the committee are men of the highest caliber and distinction in their own fields.

After presenting the methods followed for drafting the statement, Professor Montagu considers the twenty-one paragraphs of the final document. He then gives us one paragraph at a time, following each with a few pages of explanatory and interpretative comment. These are undoubtedly the most informative sections of the book and serve the reader well by giving him much of the evidence or reasoning on which the original sections are based. On the whole Professor Montagu has done a masterly job of condensation and effective writing which is, at the same time, scientifically sound. Especially significant are his discussions of the propositions that "personality and character are raceless," and that the three major divisions of the human family "were not the same in the past as they are at present, and there is every reason to believe that they will change in the future."

The author, who played a dominant role in the work of the committee, was successful in persuading them to adopt his mutual aid thesis (presented in his *On Being Human*, recently reviewed in these pages by Professor Hankins) in

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the final draft itself—"the whole of human history shows that a cooperative spirit is not only natural to man, but more deeply rooted than any self-seeking tendencies" (Paragraph 14). It would be a fascinating chapter of anthropological—and institutional—history if we could discover by what process of cooperation this was eventually adopted into the body of the text. Since the proposition is an inference from the evidence, it introduces a theme into the statement which is both secondary and subordinate to the main purpose, i.e. to give the reader universally accepted generalizations about race and culture. Not only scientists but the layman, upon reading this assertion, may wonder why, if mutual aid is a biologically founded law of nature, it has so often been inoperative. However, this is to cavil at a minor point (even though Professor Montagu himself does not regard it as such).

The Appendix includes the United Nations' Universal Declaration of Human Rights, footnote references, a short bibliography, and an index. All of these features add to its importance as a reference document. It is hoped that future printings will not list Robin Williams as McWilliams (p. 163).

R. A. SCHERMERHORN

*Western Reserve University*

*Pattern for Industrial Peace.* By WILLIAM FOOTE WHYTE. New York: Harper & Brothers, 1951. ix, 245 pp. \$3.50.

*Collective Bargaining.* By NEIL W. CHAMBERLAIN. New York: McGraw-Hill Book Co., 1951. vii, 534. \$6.00.

These two volumes represent the two main streams of present-day scholarly interest in union-management relations. Whyte's book follows an already established pattern of his research in digging down to the data of actual interaction between people in a collective bargaining situation. Chamberlain's book, on the other hand, is a mature attempt to establish generalizations about the broad patterns of institutionalized behavior which are to be found in the collective bargaining institution.

Both of these approaches are valuable and necessary. We need studies like Whyte's—intensive, concrete, close to the facts. We need also the examination of a body of institutional practices, and the generalization about them, which characterizes Chamberlain's approach. The real question for the student of union-management relations is: How do these two

approaches articulate and hang together? Or, for that matter, is there any real basis upon which an integration may be achieved?

The basic analytical framework used by Whyte was given its modern elaboration by Chapple and Arensberg, and concurrently with the publication of Whyte's book, by Homans. It views human institutional practices as the product of face-to-face interaction. Indeed, the formal organization or corporate group is nothing but the routinized interaction of its members. A short book review is no place to quarrel over fundamental methodology. It is this reviewer's belief that the principal limitation of the Whyte approach is that it fails to come to grips with the fundamental character of the formal associations of society. The most striking thing about corporate groups is their highly structured, impersonal, bureaucratic modes of administration. Such a picture hardly emerges from the Whyte analysis, and, indeed, it never can because his methodology is not designed to cope with such problems. Whyte has made the transition from the street corner gang to the collective bargaining situation without any modification in his conceptual apparatus. It might well be questioned whether the framework of concepts so useful in analysing the street corner society makes as much sense in illuminating the collective bargaining relationship.

Professor Whyte has done a masterful job in collecting and presenting the detailed data on union-management relations in a company where there was a transition from open and bitter conflict to relative peace and harmony. This case study is a valuable addition to the literature. Whyte is always mindful of the action role which he as a researcher can play. He never shies away from prescribing the remedy for some of our union-management ills. The title of this work could well be "Prescription for Industrial Peace."

Professor Chamberlain has written an excellent text book on collective bargaining. He examines, in detail, the institutional framework of union-management relations and draws out generalizations about collective bargaining which are relevant to this framework. The aim of the work is to generalize, to see the common elements in collective bargaining, wherever found. In short, Chamberlain sets himself the task of setting forth the elements of institutionalized behavior and their roots, in collective relations between labor and management. He is singularly successful in achieving this goal. His accomplishment makes clearer than countless symposia on



interdisciplinary cooperation the fact that intelligent and insightful analysis of a clearly defined problem emerges necessarily as a cross-disciplinary result. One man can achieve a comprehensive and integrated approach when the dimensions for analysis are well defined and when he is willing to see his problem as the understanding of institutional practices. Chamberlain, although an economist, has not written another labor economics. His book, therefore, has particular usefulness for industrial sociologists, and for the students of the power structure and relations in our society.

Both of these volumes deserve serious attention. They make clear how rapidly our knowledge is advancing in the area with which they deal.

ROBERT DUBIN

*University of Illinois*

*Où Va le Travail Humain?* By GEORGES FRIEDMANN. Paris: Librairie Gallimard 1950. 389 pp. 590 fr.

From its beginning the industrial revolution has had two interrelated effects within industrial enterprises: (1) an increasing specialization of the individual job at the work level, and (2) an increasing complexity of organization. I make this point because American industrial sociology has in recent years, with its studies of the social structure of industry, tended to emphasize the second effect and has somewhat neglected the first, whereas the present book tends to reverse the process and, while mentioning social structure, tends to emphasize the increasing specialization and routinization of jobs. Both effects need to be considered in a well-rounded study of modern labor. This is no criticism of M. Friedmann's book, but an effort to put it in its place in the field.

M. Friedmann, already well-known as the author of *Problèmes Humains du Machinisme Industriel*, is Professor at the *Conservatoire National des Arts et Metiers*. His present book is not a report of his own empirical research but is, in the first instance, a review. He gives us his reflections on a long voyage made to the United States in 1948, in which he was at pains to visit American factories and to establish contact with American industrial sociologists and psychologists. Americans will be especially interested in this part of his book, and will find most of his views on the American scene well-balanced and just. He goes on to review the work of various European students of industry whom he con-

siders important. He does not stop at reporting these studies but uses them as take-off points for his own reflections and conclusions on modern labor.

As I say, Friedmann concentrates on the increasing specialization and routinization of modern labor, reaching its extremity on the assembly line. His point of view is moral. The old-fashioned craftsman, or better the skilled machinist, acquired, by his expert knowledge of the material he worked in and of the theoretical foundation of the many jobs he had to do, a high degree of self-respect and intellectual culture. The latest developments in the industrial revolution have tended to make such men rare, and their qualities have tended to disappear. "The technical milieu of a factory devoted to straight-line and assembly-line production exercises, whatever be the social, geographic, or ethnic situation, a disintegrating effect on the mentality of workers, the development and use of their critical thought, their desire for initiative, and their sense of personal and collective responsibility." It is refreshing to have Friedmann talk about the dignity of man, when so many of us Americans seem to believe, without saying so, that people can be made to "adjust" to anything, and never wonder if there are some things people should not be asked to adjust to.

Friedmann's words are strong. He may well be right, but I am not sure we know enough yet to be sure he is right. To take his *bête noire*, the assembly line, we do not yet have any sufficient study of assembly-line work. Such work obviously varies in several respects, for instance, in the smoothness of organization, in the degree of possible social contact between the workers, and in the tempo of the work. We do not know in any adequate detail under what specific conditions and for what kinds of workers assembly-line work is unsatisfying or when, if ever, it is satisfying. This is not to say that Friedmann is mistaken; it is to say that his strong conclusions are not to be accepted as established.

Friedmann does not simply want a return to old-fashioned industrial conditions. Nor does he find a simple solution to his problem in a change of political and economic system, but recognizes that the conditions of modern industrial are largely the same under several different systems. For him the psychotechnician should try to make every specialized, repetitive task still less demanding in effort and attention, so that the worker can devote his interest to a pleasant social life or a piped-in cultural program. In Friedmann's view—and this seems a counsel of

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despair—work in a modern factory can never, in itself, be satisfying; therefore the hours of work must be shortened, and the worker must get through his leisure-time activities the satisfactions and cultural development he cannot get from the job. He must also receive a better education, including, besides humanistic culture, the scientific body of theory that gives meaning to his job. Finally, he must have increased participation in the control of his technical environment. This leads immediately to the problems of organization to which Friedmann gives little attention, though he is clearly not altogether satisfied with existing workers' organizations.

Though it is, through no fault of Friedmann's, based on a narrow foundation of adequate concrete studies, so that we cannot be sure how many of his judgments are well founded, this is a thoughtful, temperate, high-minded, and important book.

GEORGE C. HOMANS

*Harvard University*

*UNRRA: The History of the United Nations Relief and Rehabilitation Administration.* Three Volumes. Edited by GEORGE WOODBRIDGE. New York: Columbia University Press, 1950. lx, 1639 pp. \$15.00.

It was on November 9, 1943, "seven months before the Anglo-American landings on Northwest Continental Europe and a year and a half before the surrender of Germany but about a year after the victories at El Alamein and Stalingrad had given clear indications of the ultimate victory of the United Nations," that representatives of forty-four nations, meeting at the White House, signed an agreement creating the United Nations Relief and Rehabilitation Administration that came to be known as UNRRA.

This new international body was established by the participating nations to "plan, coordinate, administer or arrange for the administration of measures for the relief of victims of war in any area under the control of any of the United Nations through the provision of food, fuel, clothing, shelter and other basic necessities, medical and other essential services." These tasks, never clearly defined, UNRRA attempted to discharge during the following four or five years, until October 1, 1948, to be exact, when Harry E. Howell was officially appointed Administrator of Liquidation.

We have in these volumes the official history of UNRRA—the authoritative, permanently available, detailed record of its organization,

operation, problems, and accomplishments. Volumes I and II are divided into eight distinct parts, the first of which is devoted to a description of the international aspects of the organization. Starting with an account of the background and formation, it proceeds to analyze the functions and operations of the numerous and varied subdivisions of UNRRA.

The operating organization, especially its structure, its administrative and financial controls, its personnel practices and policies, and its ultimate termination are all discussed in Part Two. In the next section are treated the complex elements of the vast supply program, showing how supplies were divided among the receiving countries as well as the methods by means of which they were obtained from a remarkable variety of sources. Detailed analyses are given of the particular supply programs—food; industrial rehabilitation equipment; medical and sanitation commodities; clothing, textiles, and footwear; agricultural rehabilitation materials; and shipping.

In spite of the great variety of traits characterizing field operations, there appear to have been certain aspects common to all countries. A treatment of such common elements as agreements with different nations, the services of voluntary organizations, and special programs is presented in Part Four. There follows, in Part Five, an excellent summary account of the actual work in each European country, while in Part Six there is a similar but less comprehensive report on activities in the Far East.

The seventh part is given over to a penetrating analysis of the position of UNRRA in respect to the care of displaced persons and a description of the actual field work in their behalf. Finally, in Part Eight there is an objective, careful evaluation of the work of this extensive effort in international cooperation which took place during a period of great stress and tension.

An adequate, fifty page index, of considerable usefulness to the research student, completes the second volume. Volume III constitutes a permanent depository of much hitherto unpublished documentary material, including the basic international agreements with benefiting countries and other related materials.

It is difficult to appraise fairly and accurately the degree of success achieved by UNRRA. Its duties were not set with precision or exactitude. It was inevitably affected by the overwhelming impact of civilian and political conflict. Moreover, its close relation with and dependence on other similarly directed activities added to the

complexities inherent in its own organizational set-up.

On the other hand, there are certain facts which stand out clearly from the mass of details presented in these three volumes. The work certainly needed to be done. China, Greece, Italy, and other similarly situated nations had no foreign exchange with which to procure desperately needed commodities. In part, at least, UNRRA helped prevent widespread famines within the receiving countries. The world was aided to keep free "from devastating epidemics—a remarkable, indeed incredible achievement, if viewed in relation to the aftermaths of other great wars. The recovery of agricultural production, even in an internally disturbed country such as Greece, was very considerable." Also, industrial production was improved, essential utility services were made available, and thousands of refugees were assisted in a variety of ways.

But all did not move smoothly within this enormous agency. "As with most large organizations, relations between the component parts were not precisely defined or always happy." Its financial statements, the state of its accounts, its statistical reports, and the calibre of its personnel were not always satisfactory. At times, complete depots of military surplus were purchased resulting in the acquisition of more or less useless supplies.

Perhaps the most significant hypothesis that emerges from the over-all experience of UNRRA is that "if general agreement as to ultimate ends exists, representatives of national governments, while supporting the views of their respective governments, can, nevertheless, reach agreement regarding the practical ways of achieving the desired ends. Here and there compromises must be made, but they can be and are made; basic harmony is maintained. It is when no agreement regarding goals exists that the discussion of ways and means becomes acrimonious and violent, and rigid stands forbid all compromises."

This set of books constitutes an outstanding contribution to the rapidly growing literature dealing with contemporary world organizations. It is bound to be of considerable interest to the historian, the political scientist, the health and welfare worker, all of whom will find here many data related to their respective fields of specialization. And, of course, the sociologist is bound to be interested in this carefully compiled life history of a major social institution.

CHARLES G. CHAKERIAN

*The Hartford Seminary Foundation*

*Social Pathology: A Systematic Approach to the Theory of Sociopathic Behavior.* By EDWIN M. LEMERT. New York: McGraw-Hill Book Company, Inc., 1951. viii, 459 pp. \$4.50.

This book reinforces the welcome evidence that more sociologists are studying social deviation objectively. Part I, "Theory," develops a set of concepts for analyzing deviant or sociopathic behavior and the societal response to deviation. Part II, "Deviation and Deviants," applies the conceptual apparatus to seven areas of sociopathic behavior: blindness, speech defects, radicalism, prostitution, crime, alcoholism, and mental disorders.

Judgment of this treatise must depend mainly on how well its theoretical orientation avoids empiricism and evaluation hitherto characteristic of this field. The author postulates a differentiated role structure in which exist behavioral modalities. Culture conflict causes deviation from the modalities. Deviance may involve both overt behavioral aspects (subdivided into verbal and nonverbal) and covert symbolic aspects (attitudes). The author offers a functional classification of deviation: *individual*, originating from intra-personal attributes, e.g.: genius; *situational*, stemming from external social pressures, e.g.: prison homosexuality or depression radicalism; and *systematic*, where self-conscious deviant subcultures arise, such as that of drug addicts.

Another group of concepts hinges on the "societal reaction" to deviation (ch. 3). Reactions may range from strong approval through indifference to strong disapproval. Only the latter end of the continuum is treated in this book. Responses to sociopathic or disapproved behavior include eventual acceptance, repression, and ambivalence. In the general case the intensity of the societal reaction varies directly with the degree, amount, and visibility of the deviation (p. 54). The many factors which obviously modify this generalization are not developed by the author.

A final set of concepts relates to the "sociopathic individuation" of the deviant—how deviant roles and self-definitions are influenced by the societal reaction, and how the deviant's roles are determined by subjective and objective limits on his choices. The author defines an "adjusted" person as one whose self-definition matches his societal definition. (But is that appropriate for a personality self- and socially-recognized as experiencing conflict?)

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of this approach. The author is aware of the immensity of his task and of some of the limitations of his treatment. He freely admits, for instance, the *ad hoc* nature of much of his empirical data (p. 25). To this reviewer, the book's chief failings are its lack of (1) a systematic conception of basic or normal institutional patterns in American society; (2) a perspective of the direction of recent institutional change. An adequate institutional theory would have precluded the contradictory and evaluative statement that American culture has a preponderance of negative over positive norms because of its individualistic philosophy (p. 33). This implies a relative absence, or a random individual choice, of positive norms. Yet every social system requires as a prerequisite of its existence a set of positive values defining its basic goals and institutional means. Is not a positive normative framework implicit in the very idea of deviation?

Similarly, a systematic conception of change in American society would have averted the naive ethnocentrism which depicts gangsterism and radicalism as importations of Sicilian, Slavic, and Jewish aliens (p. 47). Doubtless immigration was one important vehicle of those particular deviant patterns. But the sociological problem is to ascertain what strains in American society made for receptivity to those deviations. What needs did they fill? What was their relation to native crime and native radicalism?

A mature theory of deviation must be an integral part of a theory of social systems and social change. This book does not achieve that theoretical integration. Systems of theory, however, develop slowly by small accretions. Here are useful tools and bricks for further building.

Each of the empirical applications in Part II follows a roughly standardized topical plan: the nature of the deviation, its differentiation (age, sex, class and other data on the deviants), its social contexts, organizations of the deviants, social visibility, the societal reaction, individuation (defining one's self as a deviant), social participation of deviants, their adjustment, and marginal categories of the deviation. Especially worthwhile are the discussions of exploitation of deviants and the occasional cross-cultural references to deviation in other societies.

The chapters on specific deviant patterns are not intended as comprehensive treatments, but as experimental exercises of the conceptual apparatus developed in Part I. Hence the theorist will be interested in this book, and the general student will gain considerable insight and in-

formation. Specialists in the several fields covered in Part II will find various bones to pick. The second word of *Jehovah's witnesses* is mistakenly capitalized throughout the treatise, although the members of that sect never do so for reasons that are sociologically very significant. The least satisfactory chapter is that on radicalism, which suffers from ideological tub-thumping and superficial moralizing. Certain minor "deviations" of the author may be noted—e.g. cumbersome word-coinings like "prisonization," and occasional bibliographic errors.

The importance of this book must be judged by its contribution to a mature theory of social systems, social change, and social deviation. It is a definite step forward on a long path.

ARTHUR K. DAVIS

Union College

*Geography in the Twentieth Century: A Study of Growth, Fields, Techniques, Aims and Trends.* Edited by GRIFFITH TAYLOR. New York: Philosophical Library, 1951. x, 630 pp. \$8.75.

The twenty different authors of this book deal, in twenty-six essays, with a wide array of subject-matters, though by no means all that from time to time appear under the rubric of geography. After an Introduction by the editor, which proves to be a rather curious dissertation in defense of determinism, there follow five scholarly and highly interesting historical chapters. The last in this series is a careful re-examination of the controversy between the *possibilism* of la Blache and the determinism of Ratzel, Semple, and others. Part II, bearing the title "The Environment as a Factor," includes a number of technical papers on such problems as geomorphology, meteorology, climate, and soil, two fascinating resums of polar explorations, discussions of the problems of pioneer and tropical settlement, and a brief description of the British land use survey of 1932. In Part III, designated "The Fields of Geography," are grouped a miscellany of papers which vary from topics such as "Geography Is a Practical Subject" and "Geography in Practice in the Federal Government" to "Racial Geography," "The Sociological Aspects of Geography," and "Geopolitics and Geopacifism."

If the content of the book is representative of the field, geography shows many of the symptoms of immaturity. Ostentatious claims (e.g.

"... and geography is emphatically the discipline which can instruct mankind objectively and visually as to the problems of our troubled world." Griffith Taylor, p. 27) are countered by modest statements of problem (e.g. L. Dudley Stamp's "Land Use Surveys with Special Reference to Britain," ch. XVI). Superficiality (e.g. E. W. Gilbert's "Geography and Regionalism," ch. XV, and Griffith Taylor's "Urban Geography," ch. XXI) appears alongside intensive application to specific tasks (e.g. D. F. Putnam's "Soils and Their Geographic Significance," ch. X). Again, intellectual confusion (e.g. "... demands, ... wants, ... ideas. ... These are all geographical ... because they help us to understand the interrelationships between folk and place." J. W. Watson, p. 471) is juxtaposed with careful reasoning (e.g. G. Tatham's "Environmentalism and Possibilism," ch. VI).

On second thought, perhaps the less laudable of these symptoms point not to immaturity but to a tendency on the part of certain geographers to overreach their subject-matter, to fabricate a social science from the original concern for topography, climate, drainage systems, and the

like. Dissatisfaction with taxonomy and cartography—and these seem to be the distinctive tasks of geography—is understandable. But training confined to such skills is grossly inadequate for explaining human behavior. Nor does dissatisfaction, if indeed that be the irritant, grant a license to subsume everything under geography that is susceptible to correlation with the features of the physical landscape. In short, it seems to this reviewer that many geographers are caught in a dilemma. Theirs is a very restricted subject, yet they wish to operate, *qua* geographers, in all types of human behavior problems and subject-matters. There is no objection to their doing the latter provided they are properly trained, but to insist that everything they observe is geographical simply because they have looked upon it or because it is "on the land" is to inject a pernicious argument into what otherwise may be a good work. It would be wiser to forget traditional academic labels when dealing with certain kinds of problems, especially problems of immediate practical concern.

AMOS H. HAWLEY

*University of Michigan*

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## BOOK NOTES

*Social and Psychological Factors Affecting Fertility, Vol. II; The Intensive Study: Purpose, Scope, Methods and Partial Results.* Edited by CLYDE V. KISER and P. K. WHELPTON. New York: Milbank Memorial Fund, 1951. vi, 327 pp. \$1.00.

This volume places within a single cover Parts IV-X of the continuing Indianapolis Study, each of which appeared separately in issues of the Milbank Memorial Fund *Quarterly* between 1945 and 1950. (Parts I-III were bound and issued as Vol. I, "The Household Survey in Indianapolis," in 1946.) The first two papers, by Whelpton and Kiser, present narrative accounts of the plan of the intensive study and a discussion of the sampling design and field work. It is obvious from these two papers that considerable time and effort were devoted to the construction of a careful study, yet it is regrettable that the methodological detail presented is not always sufficient for critical consideration on the part of the reader. For example, though we are told how many questions relate to each of the hypotheses the study was designed to test, the precise questions and their location in the schedule are not revealed. Perhaps these will appear in a forthcoming volume.

The remaining papers—four by Whelpton and Kiser, one by Robert B. Reed, and one by Ronald Freedman and Whelpton—present some results of the study. Among the most important findings are: observed differences in fertility were not due to differences in fecundity but rather to planning with regard to family size; and marital adjustment tends to increase with an increase in the positive control of fertility. Perhaps the most important conclusion of the study is that while socio-economic status and fertility are inversely related in groups of least effective planning, they tend to be directly related in the most effective planning groups. (The authors are always careful to point out that their findings relate only to the group of Protestant women which were studied in Indianapolis.)

Because these articles appeared separately over a period of about five years and were not re-edited as a single volume, considerable repetition appears in the statements of the major purposes and plans of the study. The

tabular presentation is clear and generally uncomplicated, though the table forms are not always consistent.

This is an important and useful study. Though still incomplete, the first two volumes should prove extremely valuable to students of demography, sociology and social psychology.—ELEANOR BERNERT

*The Jews and Modern Capitalism.* By WERNER SOMBART (Translated by M. EPSTEIN, with an introduction by BERT F. HOSELITZ). Glencoe, Ill.: The Free Press, 1951. xlii, 402 pp. \$4.50.

In the introduction to this translation, Bert F. Hoselitz brings up the question why it seemed desirable to publish a new edition of this book. He mentions two reasons. The first one is that, in spite of all its defects, it deals with all the crucial problems of the role played by the Jews in the development of modern capitalism. In the second place, Hoselitz regards it as a classic in the field of Jewish social relations.

Both these remarks might deserve to be met with a certain caution. If the term "classic" conveys excellence in regard to analytical powers as well as to formal expression, Sombart's work hardly merits a place in this category. It is a standard example of how "facts" can be made to fit an idea. If the idea itself is woven into a consistent and logically attractive theory, one would still feel that here at least was an intellectual achievement of some importance. Sombart, however, never even attempts to define the problem he is dealing with. He simply equalizes rational, economic mentality with Jewish mentality; then assumes that commerce and capitalism—he nowhere differentiates clearly between the two—presuppose rationalism, and *ecco*, there is the correlation. A few examples suffice: "In the face of the fact, is there not some justification for the opinion that the United States owe their very existence to the Jews? And if this be so, how much more can it be asserted that Jewish influence made the United States just what they are—that is, American? For what we call Americanism is nothing else, if we may say so [sic!], than the Jewish spirit distilled" (pp. 43-44).

Another example of Sombart's reasoning: "It was also true of Amsterdam [Sombart is dealing with Jewish influence in finance], though we must add that the first mention of Jews in that capacity was not until the end of the 17th century. Despite this, however, I believe that we shall be safe in assuming that previous to that date also they were influential bill-brokers" (p. 83).

Sombart assumes more things: the rationality of the Jewish mind (he has never heard of the Jewish mystics) is explained from the fact that the Jews in their pastoral stage of 4000 years ago were given to counting sheep. Counting sheep and counting money are to Sombart about the same thing, albeit not on a sleepless night.

Nevertheless, it is worthwhile to have this book in a good translation if not as a document of, then at least for, sociological study. Sometimes, one really feels that the social sciences have progressed and that such emotional and biased studies which fall short of reasonable theoretical requirements would be impossible today.—B. LANDHEER

*The Education of the Mexican Nation.* By GEORGE F. KNELLER. New York: Columbia University Press, 1950. xi, 258 pp. \$3.50.

This book is not "the first comprehensive work on education in Mexico ever published" as stated in the first sentence of the jacket blurb. The book entitled *Mexico: A Revolution by Education*, written in 1936 by George I. Sanchez, is more "comprehensive"; and there have been others of equal importance. Nevertheless, the present work is a valuable contribution to the literature on this subject and brings many of the recent developments up to date.

The book is divided into eight chapters as follows: The Land and the People; The Struggle for Education; Contemporary Thought and Organization; The Early Years; The Education of Adolescents; The Teacher; Higher Education; and Education in a Total Culture.

Especially important are the chapters dealing with contemporary Mexican thought on educational problems and the descriptions of the organization and functioning of the school systems from the kindergarten to higher education.

The work provides a great deal of useful information for students of Latin America but there are a few noticeable gaps. For instance, the author devotes only slightly more than one

page to Mexico's Cultural Missions, although these have received international attention and have been considered important enough to warrant special study by UNESCO as an example of what might be tried in other underdeveloped countries.—N. L. WHETTEN

*Journey for Our Time: The Journals of the Marquis de Custine.* Translated by PHYLLIS PENN KOHLER. New York: Pellegrini & Cudahy, 1951. viii, 338 pp. \$4.00.

The Marquis de Custine, a French Legitimist and experienced political observer and writer, made a journey to Russia in 1839 to acquire first-hand knowledge on a monarchical regime at work. His secret ambition was to collect enough factual material to support his own unfavorable opinion of Western parliamentarism. In other words, he undertook his journey as a "fellow traveler." Yet, he came back a partisan of "constitutions" and "representative governments." His visit to Russia taught him a lesson in the inherent evils that permeate a monarchical despotism.

The translation of Custine's journals into English over a century after their appearance in France is no wasted effort. The great merit of this book lies in the fact that it not only depicts the Russian socio-political reality during the 1830's, but also that it explains essentially such basic attributes of the Soviet system as permanent mobilization, all-permeating bureaucracy, organized fear, ascription of divine powers to autocratic rulers, rigid restrictions on freedom of discussion, and officially actuated distrust in everything Western.

The students of Soviet society will welcome this book, despite the fact that it throws light only on social continuities. For the understanding of Soviet society as a historically unique structure they must turn to other sources.—A. VUCINICH

*Cooperation among Animals: with Human Implications* [A Revised and Amplified Edition of *The Social Life of Animals*]. By W. C. ALLEE. New York: Henry Schuman, 1951. xiv, 233 pp. \$3.50.

Summary of "a general sociology, which allows human social relations to be viewed in part as the peculiar human development of social tendencies . . . among . . . animals generally" (p. 148). But there is an important disclaimer: "our motivation has not been . . . an oblique

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attack upon human social relations" (p. 147).

Data presented indicate that numbers enhance safety among any species of organisms—up to a point; that numbers of organisms improve the survival rate of their successors by conditioning an unfavorable medium to reduce its ill effects; that in populations of optimum density survival is promoted by retardation of certain disorganizing processes and acceleration of beneficial processes. Evolution of social animals has occurred again and again independently; and "no free-living animal is solitary throughout its life history" (p. 212). "I regard those groups in which animals confer distinct survival values upon each other as being at least partly social" (p. 154). The hen is no cynosure for man: "real leadership . . . [is] . . . not . . . a peck order, which . . . does not imply leadership at all" (p. 147).

The chapter on human implications is somewhat of a *non sequitur* though the author disarmingly admits his personal convictions and refrains from deducing the U.N. from biology.—DOUGLAS G. HARING

*Executive Action.* By EDMUND P. LEARNED, DAVID N. ULRICH, and DONALD R. BOOZ. Cambridge: Harvard University Graduate School of Business Administration, 1951. xiv, 218 pp. \$3.25.

This book reviews itself: "[It] is addressed to business executives and others . . . interested in

the practical aspects of business administration" (p. 3). "Any reader, business executive or otherwise, who customarily reads with an eye for rigorous research method, precise logic, statistical proof, or other indices of certainty may find that this does not come within his range of interest. Nor will the reader find here any startling conclusions, any discoveries or formulas" (p. 4). Within the limitations suggested by these quotations, the following statement of purpose is pertinent: "The basic objective of this book is not to set forth techniques but to help light up the unexplored territory of human activities through which the executive must pass" (p. 86).

Conclusions: (1) the purpose of building business organizations is a continuous one; (2) in most companies the task of top management is no longer a one-man job; (3) executives could place more reliance on subordinates; (4) there is no substitute for face-to-face contacts as a means of communications; and (5) most executives are capable of developing greater understanding of the "human context" of their work. These conclusions rest upon a number of accounts of situations involving executives. Many foremen and minor executives will recognize their familiar ring. They have read them before, as applied to their own statuses, and have often suggested that the "men up top" should be similarly instructed.—JOHN S. ELLSWORTH, JR.

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